Against all noise
On noise-robust strategies in the emergence of cooperation

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Abstract

For cooperation to evolve via direct reciprocity, individuals must track their partners' behaviour to avoid exploitation. Noise (i.e., memory errors or perception/decision errors) compromises tracking, however. In my thesis, I investigate whether strategies proposed to model human behaviour are cognitively feasible and how they cope with noise, and explore feasible noise-robust alternatives.

Tit-For-Tat, the most prominent example of 1-step memory strategies, is not robust to noise, because even little noise decreases its success. Since noise is quite common in everyday life, Tit-For-Tat is not an ideal candidate to model human behaviour. Chapter 1 showed that participants, when asked to remember their partners' previous behaviour (1-step memory), had high memory error rates. In an evolutionary simulation, these rates let cooperation vanish. Remembering a partners' previous behaviour is neither noise-robust nor cognitively feasible.

In Chapter 2, I investigated whether people use the cognitively more feasible strategy of categorizing partners into types, distinguishing cooperators and cheaters. Compared to remembering each partners' previous behaviour, this would reduce memory effort. The results indicate that people differentiate partner types and adjust their strategy to the proportion of types in their environment.

Chapter 3 explored strategies that model the process of categorizing partners into types by building an impression. In a simulation, impression-based strategies were more robust to noise in maintaining cooperation than 1-step memory strategies. A cross-validation of strategies on data from Chapter 2 confirmed that impression-based strategies better predict participants' behaviour than 1-step memory strategies. The winner of the simulation and the cross-validation were non-contingent strategies, though, indicating that people use cognitively even simpler noise-robust strategies.

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Introduction

Imagine you collaborated with a colleague on a project. It was your last project working for the company before you will head off to a new job, and you put hard work into it. You have never collaborated with this colleague before, and he will leave the company, too. The boss is not satisfied with the quality and wants to talk to you both individually to search for reasons and find the one to blame. Imagine further that the two of you just have the choice between “cooperating” and “defecting” with the other, when “cooperating” means to remain silent and “defecting” is to blame the other one. You have no opportunity of talking to each other, and even if the other was at the boss’ first, you do not have a chance getting to know what he decided to do before you go talking to the boss yourself. Depending on what your colleague (called “partner”) and you (called “player”) decide to do, you receive payoff mirroring the benefits.

There are four possible combinations: First, you defect with your colleague and blame him, whereas he cooperates and remains silent. Thereby, you are looking pretty good in the eyes of your boss (usually represented by a payoff of 5); your colleague attracts the whole resentment. Second, your colleague and you defect with each other and blame one another. The boss will think that none of you is completely innocent when it comes to the quality of the work and call both of you to account (payoff: 1). Third, your colleague and you cooperate and remain silent what concerns the one to blame. Your boss will be insecure and teach both of you at least a little lesson (payoff: 3). Fourth, you protect your colleague and remain silent, whereas he blames you. You will have to carry the whole damage yourself (payoff: 0), whereas your colleague gets away without a penalty. You will want to save yourself and defect, hoping that the other one will not (payoff: 5; called “T” for temptation), but if your colleague comes to the same conclusion, you will end up sharing the boss’ conviction (payoff: 1; called “P” for punishment). So you might think of cooperating, hoping that the other one will, too (payoff: 3; called “R” for reward), but you risk getting all the blame (payoff: 0; called “S” for sucker). What would you do?

Considering that you have no strings attached to the colleague or the company, you might defect. Now imagine this situation in your new company with a colleague you will probably collaborate with repeatedly. There is hardly a doubt that you will cooperate. The prospect of repeated interactions does not only impose discipline with regards to the temptation but also offers the possibility to punish the partner if he exploited you. Still, each cooperation bares the risk of sucking which makes the situation a fascinating research subject. Since the early 1950s, researchers have used this paradigm (called Prisoner's Dilemma after the original example; e.g., Luce & Raiffa, 1957) to investigate the emergence of cooperation. It only qualifies as a Prisoner's Dilemma game if the payoffs meet the size order: $T > R > P > S$ and if alternating defection and cooperation is, on average, not more beneficial than mutual cooperation: $2R > (T + S)$.

In his seminal work, Axelrod (1984) asked which strategies people could use to gain the best possible payoff playing repeated Prisoner's Dilemma games (called Iterated Prisoner's Dilemma) against a variety of partners and when continuation of the interaction is uncertain. In several of his studies (1980a, 1980b, 1987), Tit-For-Tat (TFT) won, a strategy that begins by cooperating and then reciprocates the partner's previous move. Although celebrated because of its intuitive eye-for-an-eye style, there is hardly evidence for TFT outside of theoretical studies (e.g., Oskamp, 1971, Wilson, 1971, König, 1988, Opp, 1988, Masters & Waite, 1990). How is that? Noise had been neglected in its design. There are different sources of noise. Noise can come along with the requirements of the strategy (e.g., memory, amount of computation) or result from the decision maker (e.g., perception, decision). Whether a decision maker implements a decision in compliance with a strategy depends on both sources. One can ask for each strategy whether its requirements are cognitively feasible and how it copes with noise originating from the decision maker.

Authors soon established that TFT is not robust to noise from the decision maker—one incorrectly implemented move diminishes its success (e.g., Molander, 1985). As noise-robust alternatives, authors proposed, among others, a generous version of TFT (begin with cooperation, reciprocate the partner's move, but after defection cooperate with some probability; e.g., Nowak & Sigmund, 1992) and Win-Stay, Lose-Shift (begin with cooperation and cooperate after mutual cooperation or mutual defection, otherwise defect; e.g., Nowak & Sigmund, 1993). Being both members of the 1-step memory family just like TFT, they, too, require to remember a partner's previous move. Whereas generous TFT and Win-Stay, Lose-Shift proposed more robustness to noise from the decision maker, they did not take into account noise from the design of the strategy. Is the requirement to remember each partner's previous move cognitively feasible? If not, one would have to explore a different strategical design to find strategies that master both sources of noise. These considerations determine the content of my thesis.

Chapter 1 investigates whether remembering each partner's previous move, as 1-step memory strategies require, is cognitively feasible and whether these strategies are in fact noise-robust as was proposed. Chapters 2 and 3 explore cognitively feasible and noise-robust strategies to explain the emergence of cooperation. These strategies take into account yet another source of noise: the interaction partner. To distinguish between intentional and unintentional partner behaviour, the strategies do not just consider the partner's previous move but his typical behaviour.

Are Tit-For-Tat and its relatives cognitively feasible and noise-robust?

When Axelrod (1984) wanted to find the best strategy for the Iterated Prisoner's Dilemma, the proposed strategies were designed to gain the highest payoff against a variety of partners. To achieve this, some of the strategies used complex calculations to predict the partner's next move or a specifically worked-out agenda for what to do at which point in the encounter. This is an example strategy (Axelrod, 1980a):

> GRAASKAMP […] plays tit for tat for 50 moves, defects once, plays tit for tat for another 5 moves, and then examines the history of the game so far. Its defection on move 51 allows it to recognize its own twin and be cooperative with it. Similarly, it can check to see if the other player seems to be TIT FOR TAT or another program it recognizes. If so, it plays the rest of the game in an appropriate way. If its score is not very good, it suspects (perhaps incorrectly) that it is playing RANDOM and defects for the rest of the game. Otherwise it continues to play tit for tat, but throws in a defection every five to fifteen moves. (p. 12)

These strategies did not conform to the concept of a boundedly rational decision maker who finds a good decision with limited time, limited knowledge, and limited cognitive capacities (Simon, 1956, Selten, 2001). To the general surprise, with TFT, a strategy won that adheres to a comparatively simple rule: Begin with cooperation and reciprocate your partner's previous move. TFT did not always gain the highest payoff but, on average, a good payoff against a variety of partners. Moreover, compared to other strategies (see GRAASKAMP above), it uses limited information (the partner's previous move) and does not have high cognitive requirements. TFT is a simple strategy. Or is it?

TFT is the most prominent example of 1-step memory strategies, and for it to work, TFT has to remember the partner's previous move exactly, interpret it exactly, and reciprocate it exactly. Two opponents repeatedly playing TFT gain the highest average payoff and establish mutual cooperation, but a single deviation, a single incorrectly implemented move, destroys this success, because both opponents will in turn reciprocate the defective move. As a result, alternately gaining Temptation's and Sucker's payoff gives less than the averaged Reward (Axelrod, 1984). TFT is not robust to perception or decision errors.

Axelrod (1980a) had already remarked TFT's susceptibility to punishment by being too easily provoked and even uttered "[...] a warning against the facile belief than [sic!] an eye for an eye is necessarily the best strategy". The degree of forgiveness to avoid this negative effect, Axelrod wrote, would depend on the environment (i.e., the competing strategies). Whereas TFT's degree of forgiveness (i.e., robustness to noise—or lack thereof) was adaptive in Axelrod's (1984) tournament environments, it could fail in a different environment. In interactions between humans, for example, more noise is to be expected, asking for noise-robust strategies to maintain cooperation.

Whereas Axelrod (1984) had referred to TFT's susceptibility to noise from the decision maker (perception/decision errors), he had not studied the feasibility of TFT's requirements. Memory is one of the cognitive capacities needed for reciprocity (which TFT tries to model; Stevens, Cushman & Hauser, 2005). Are humans capable of remembering each partner's previous move? In human interactions, noise in the form of memory errors could arise when players do not meet their partners one (repeatedly) after the other, as assumed in theoretical work (e.g., Axelrod, 1984). Under more realistic conditions, when players meet their partners randomly (as in Winkler, Jonas & Rudolph, 2008), remembering each partners' previous move would become an enormous task. The probability of committing but a single error would increase and demonstrate TFT's susceptibility to noise. The first hypothesis of Chapter 1, therefore, is that TFT and its 1-step memory relatives are not simple strategies with prerequisites meeting the capacities of a boundedly rational decision maker.

Participants met several partners repeatedly but randomly and observed their behaviour. Then, participants were asked to recall their partners' previous move—just like 1-step memory strategies require. This procedure would reveal whether participants were theoretically capable of using TFT and its relatives. Also, it would give an idea of realistic noise levels. Previous simulations had tested the robustness to noise of TFT and other 1-step memory strategies. In these simulations, noise was not defined as memory error. Nevertheless, I draw on them for comparison, because this distinction in content is irrelevant for a player in the simulation: He commits an error—regardless of the source. Noise in these previous studies ranged—except for a few analytical derivations (Molander, 1985, Boyd, 1989, Bendor, 1993)—from $p = 0.001–0.2$ to do the opposite move (Donninger, 1986, Müller, 1987, Lindgren. 1991, Nowak & Sigmund, 1993, Wu & Axelrod, 1995, Lomborg, 1996, Miller, 1996, Sherratt & Roberts, 1999, Wakano & Yamamura, 2001, Hruschka & Henrich, 2006, Anh, Pereira & Santos, 2011). Are these levels of noise to be expected in human interactions? In the second part of Chapter 1, simulations should show how noise-robust TFT and other 1-step memory strategies would be at the experimentally derived noise levels and whether they were able to maintain cooperation. Find the results in Chapter 1.

Is the strategy of assigning partner types ecologically rational?

Despite TFT's susceptibility to noise and although its 1-step memory design probably does not model human behaviour, the appealing is the idea of reciprocity behind it. Not getting exploited but also reciprocating cooperation allows for the best average payoff against a variety of partners. Moreover, reciprocity can establish cooperation as the condition both players, on the long run, benefit most from (Trivers, 1971, but see Rothstein & Pierotti, 1988, on the debate of the correct term). With reciprocity as modelled by TFT, however, noise would cause the player to remember incorrectly at times, perceive incorrectly, or reciprocate incorrectly. This resulted in unprotected exploitation by defecting partners or needless defections with cooperating partners. How could someone forego this? As outlined in the previous section, this does not call for a strategy considering more information, employing more computation, and consequently using more time to take into account all possible partner's moves (Hertwig & Todd, 2003). Instead, I am looking for a strategy applicable by a boundedly rational decision maker.

When asking participants of Chapter 1's experiment about their strategy to remember their partners' moves, some participants reported that they ignored part of the information and concentrated on one partner behaviour, inferring the other probably by means of elimination. One participant wrote: "I only memorized names of cooperators. If a name came up that did not belong to the cooperators, I concluded that he did not cooperate". Because the computer partners in Chapter 1's experiment behaved randomly, participants concentrating on one behaviour had to constantly update the list of partners to memorize. In contrast, in everyday interactions, partners probably adhere to strategies that result in a pattern or general tendency of behaviour and may be categorized into types (e.g., cooperators, defectors). Concentrating on one type in such an environment involved a lot less updating. In general, assigning types to interaction partners avoids having to remember every previous move, thereby reducing memory requirements. Judging by the type, a player would not be fooled by defectors' occasional cooperative moves and excuse cooperators' occasional defections, thereby offering robustness to noise from the partner. Preferentially remembering one type could reduce memory requirements even more, resulting in yet more robustness in regards to that source of noise.

Which type did participants concentrate on? Is it more advantageous to memorize defectors for protecting against exploitation, or lies more benefit in securing mutual cooperation by sticking to cooperators? About equally many participants from Chapter 1's experiment reported they focused on cooperators as reported they focused on defectors. Based on the benefits and costs imposed by the payoff matrix, some participants apparently concluded that they should keep an eye on defectors. Or was an adaptive cheater-detection mechanism at work (Cosmides & Tooby, 1989)? Which type to remember could also depend on the environment (in this case, the proportion of defectors and cooperators), as remembering defectors in an environment with a majority of defectors does not reduce memory requirements much. Finding not only boundedly but also ecologically rational strategies (i.e., adapted to an environment; Simon, 1956, Todd & Gigerenzer, 2012) is the subject of investigation in Chapter 2.

Are impression-based strategies cognitively feasible and noise-robust alternatives to 1-step memory strategies?

When Axelrod (1984) described that even two TFT opponents may be caught in alternating defections, I have so far assumed that this effect was caused by perception or decision errors in the player. There is a third source of noise, however: the interaction partner. He is prone to the same noise sources as the player and could implement an incorrect move unintentionally. To prepare against this form of noise, it would be advantageous to have more information about the partner's behaviour. This would allow to distinguish intentional from unintentional moves.

Identifying TFT as a "successful strategy in a highly constrained and uniform universe", Cosmides and Tooby (1989) suggested a list of information a strategy should consider before making a decision:

> 1) the number of transactions one has had with that individual in the past, 2) how he or she behaved in those transactions (reputation), 3) the size of payoffs to both parties in previous transactions, 4) whether his or her tendency to cheat varied with the size of the payoff involved, 5) whether the conditions governing his or her tendency to cheat have been shifting over time, 6) his or her aggressive formidability, 7) how likely one is to meet that individual in the future (e.g., one party is moving away or likely to die soon), and 8) whether one has accepted a past benefit but has not reciprocated yet. (p. 62)

They continue by saying that “Information about one’s history of interaction with a particular person ought to be ‘filed’ with that person’s ‘identity’ and activated quickly and effortlessly when an exchange-relevant situation with that person arises”. The previous section mentioned a strategy combining many of the suggested information and showing a way how to quickly access them. By assigning partner types, a player could be as successful as TFT against a variety of partners. Instead of reciprocating single moves, the player would reciprocate typical behaviour. Because assigning types reduces memory requirements and decreases the probability of reciprocating atypical behaviour, such a strategy would be more robust to noise than 1-step memory strategies. This is the first hypothesis tested in Chapter 3. Whereas this hypothesis establishes the idea of partner types and their advantage in contrast to 1-step memory strategies, I have not yet examined how types come about. Chapter 3 investigates the process of categorizing partners into types by building an impression.

Other authors have dealt with partner types and how to assess them. In studies of indirect reciprocity, a player decides how to behave with a partner based on the reputation, an image score, of the latter (e.g., Nowak & Sigmund, 1998). This score is accumulated through interactions with third parties, though. How generalizable is that information when it comes to the player’s own encounter with the partner and how reliable is it considering noise from the source of that information (Boerlijst, Nowak & Sigmund, 1997)? In contrast, finding out about a partner’s type during interactions with him would reflect personal experience with the partner and eliminate the additional source of noise from the third party. This idea of impression building underlay the design of Chapter 2’s experiment, and it will underlie the design of impression-based strategies in Chapter 3.

In Aktipis’ (2006) partner type model, a player categorizes his partner on the basis of one interaction. Whereas this experience is certainly personal, it does not differ from the concept of

1-step memory strategies. If the player perceives this one move incorrectly, he will treat the partner inappropriately in the coming encounters. As in Chapter 2’s experiment, my collaborators and I envisioned impression building as a continuing process based on repeated interactions. Realizing a partner’s typical behaviour would allow to ignore part of the information, namely atypical behaviour, and be more robust to noise from the partner.

Handling more information than the previous move and combining it into an impression can mean to deviate from the idea of keeping one information per partner in mind. Still, considering more information does not have to interfere with the concept of a boundedly rational decision maker. It is not the mere quantity of information but its quality that counts. When asking participants of Chapter 1’s experiment about their strategy to remember their partner’s moves, the majority revealed that they did not construct an abstract memory account—probably because they reached the limits of memory capacity at one point. Instead, they used mnemonics and tried to link facial features to the behaviour, assigned memories of friends with the same names, or even characterizing stories. This technique enables participants in memory competitions to memorize hundreds of numbers (e.g., Levin, Levin, Glasman & Nordwall, 1992, on the effectiveness of mnemonics). Just like participants in Chapter 1’s experiment added information (mnemonics) to assign more meaning to abstract items in order to reduce memory requirements, humans could add information (and combine into a partner’s impression) to assign more meaning to their partners’ behaviour.

I could imagine different possibilities how humans arrive at an impression, resulting in different impression-based strategies. What differentiates these strategies and whether they are more robust to noise than 1-step memory strategies is the subject of the first part of Chapter 3. The second part will complement the theoretical considerations by investigating whether impression-based strategies can predict participants’ behaviour.

Chapter 1
Forgetting constrains the emergence of cooperative decision strategies

Preliminary note

Chapter 1 is based on the paper with the same title that developed in collaboration with Jeffrey R. Stevens, Lael J. Schooler, and Jörg Rieskamp (Stevens, Volstorf, Schooler & Rieskamp, 2011). To comply with the rest of the thesis, I changed American into British English and adapted the usage of terms (e.g., "memory-1" or "memory-2 strategies" became "1-step" or "2-step memory strategies"). Also, I adapted the numbering of tables and figures to be chapter specific. Available with the original paper are the participants' data, the R code to analyse them, and the Pascal code to run the simulations. Since these materials would go beyond the scope of this book, I omit them here and refer the interested reader to the source at http://www.frontiersin.org/Cognitive_Science/10.3389/fpsyg.2010.00235/full.

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Abstract

Theoretical studies of cooperative behaviour have focused on decision strategies that depend on a partner's last choices. The findings from this work assume that players accurately remember past actions. The kind of memory that these strategies employ, however, does not reflect what we know about memory. Here, we show that human memory may not meet the requirements needed to use these strategies. When asked to recall the previous behaviour of simulated partners in a cooperative memory task, participants performed poorly, making errors in 10–24% of the trials. Participants made more errors when required to track more partners. We conducted agent-based simulations to evaluate how well cooperative strategies cope with error. These simulations suggest that, even with few errors, cooperation could not be maintained at the error rates demonstrated by our participants. Our results indicate that the strategies typically used in the study of cooperation likely do not reflect the underlying cognitive capacities used by humans and other animals in social interactions. By including unrealistic assumptions about cognition, theoretical models may have overestimated the robustness of the existing cooperative strategies. To remedy this, future models should incorporate what we know about cognition.

Introduction

Theoretical analyses have demonstrated that cooperation can evolve in situations in which individuals interact repeatedly and their behaviour depends on other's and/or their own past behaviour (Axelrod & Hamilton, 1981, Nowak, 2006). For instance, the celebrated decision strategy Tit-For-Tat (TFT) cooperates on the first move with a partner and then copies the partner's single last choice (cooperate or defect) on all subsequent interactions (Rapoport & Chammah, 1965, Axelrod & Hamilton, 1981). This and similar strategies, such as generous TFT (gTFT), contrite TFT (cTFT), Tit-For-Two-Tats (TF2T), and Win-Stay, Lose-Shift (WSLS; Boyd, 1989, Kraines & Kraines, 1989, Nowak & Sigmund, 1992), have dominated theoretical studies of cooperation for the last 30 years. Despite their dominance in the theoretical work, the assumptions about the underlying cognition required to implement these strategies have not been adequately tested.

Thus, there is a critical gap between the theoretical work on which decision strategies can maintain cooperation and the empirical work on what strategies individuals actually use. To bridge this gap, we must test whether the cognitive capacities required to implement strategies are psychologically plausible (Stephens, McLinn & Stevens, 2002, Hammerstein, 2003, Stevens & Hauser, 2004, Stevens et al., 2005, Todd & Gigerenzer, 2007). Here, we investigate one of these cognitive capacities: memory for past actions. We ask whether existing strategies make reasonable assumptions about memory or whether problems associated with forgetting could constrain the emergence of these cooperative strategies.

Memory represents a primary cognitive capacity needed for strategies in social interactions that depend on past behaviour. The strategies tested in the literature for social interaction make different memory requests. The so-called 1-step memory strategies require that players accurately remember the single last choice from each partner. Two-step memory strategies require accurate memory for the last two choices. Humans and other animals, however, sometimes forget. If an individual cannot remember the past action of an interaction partner, then he or she cannot apply a strategy that relies on this knowledge.

In contrast to the existing cooperative strategies, our memory does not work like computer memory, filing away pieces of information for flawless retrieval later. Instead, our memory functions more like how a search engine retrieves information from the internet, with memory records associated to retrieval cues (Estes, 1955, Anderson, Bothell, Byrne, Douglass, Lebiere & Qin, 2004), much like how websites are indexed by keywords. This associative nature of memory leads to problems of interference, in which cues become associated with many memories, hindering the retrieval of the information sought. Our memory suffers from both proactive interference, in which old memories disrupt the retrieval of new information, and retroactive interference, in which new memories disrupt retrieval of old information.

Despite its central importance, the role of memory in cooperation has received little attention in the existing literature. In one of the few studies to explore memory and cooperation, Milinski and Wedekind (1998) examined the effects of working memory on cooperation by giving half of their participants a working memory task between interactions. They found that without the memory task, participants seemed to use a more complicated 2-step memory strategy, whereas with the memory interference, they used a simpler 1-step memory, TFT-like strategy. Winkler et al. (2008) introduced multiple partners to track, as well as varied the interaction pattern between repeatedly interacting with the same partner before switching to a new one or randomly interacting with partners. When randomly interacting with partners, participants with better recall of biographical information about their partners received higher payoffs in the cooperative games—better memory abilities at the individual level resulted in higher payoffs. These studies either measured or manipulated memory performance for information outside of the cooperative situation. Here, we test the role of memory for partners' previous actions on cooperation.

Given the nature of memory, we ask whether existing decision strategies that promote cooperation (such as TFT and its variants) are cognitively feasible. We explore whether humans have the memory capacity required to implement these strategies. Thus, we are asking whether individuals *can* use strategies like TFT, not whether they *do* use these strategies. Do they have the requisite cognitive capacity? To address this capacity question, we designed a memory experiment that tested the role of memory interference on the ability to recall past actions. Though this study does not mimic real-world cooperative situations, it is not meant to. Our experimental design replicates the conditions under which 1- and 2-step memory strategies should work in order to test the underlying cognitive assumptions of these strategies.

We conducted an experiment with human participants, in which a series of simulated partners chose to cooperate or defect. We measured participants' memory accuracy in recalling each partner's last action. To test the effects of memory interference on cooperation, we implemented two experimental manipulations. First, we varied the number of simulated partners, which is critical when interactions between different partners are interleaved (e.g., partner A, partner B, partner C, partner A, etc.). In this case, an individual may forget a specific partner's previous behaviour due to the intervening interactions interfering with the retrieval of the memory; more partners result in more retroactive interference. Second, we varied the number of interactions with each partner, because more previous interactions might interfere with the ability to recall only the single last interaction (proactive interference). From these manipulations, we can estimate how memory errors respond to increases in proactive and retroactive interference.

Estimates of memory accuracy alone, however, do not demonstrate the complete role of memory in cooperation. We must also test how well specific decision strategies cope with error caused by misremembering a partner's last actions. For instance, TFT's performance decreases when errors exist, because mistakenly defecting results in the lower payoffs of mutual defection (Molander, 1985). A more forgiving form of TFT called cTFT (Boyd, 1989) outperforms TFT when individuals make errors. Although a few strategies have been tested over a few error rates (e.g., Stephens, Nishimura & Toyer, 1995, Wu & Axelrod, 1995, Rieskamp & Todd, 2006), to our knowledge there exists no comprehensive treatment of error on the 1- and 2-step memory strategies. We used agent-based simulations to systematically analyse the success of several strategies proposed in the literature across a broader range of error rates. With these and the human memory results in hand, we can determine whether currently proposed decision strategies provide adequate models of cooperative behaviour.

Cooperative memory experiment

Methods

We recruited 216 participants (age: mean ± *SD* = 25.4 ± 3.2 years, range = 18–36 years) drawn from Berlin universities via the Max Planck Institute for Human Development participant pool. We prepared all materials in German and programmed the experiment in E-prime experimental software (Schneider, Eschmann & Zuccolotto, 2002a). The programme began by asking participants to provide demographic data (sex, age, educational level, occupation, college major).

Before beginning the experiment, participants received a paper copy of instructions (see Participant Instructions in Appendix Document A1) describing the goal of the task: recall the last action (cooperate or not cooperate) for each simulated partner. Participants returned the instructions to the experimenter before continuing to avoid giving them a means to record information during the task. A practice phase familiarized participants with the experiment. The practice phase was identical to the actual experimental session, except (1) it used fewer trials in a fixed order for all participants (three partners with four interactions each and six partners with three interactions each), (2) it included only female partners (the experimental phases included only male partners), and (3) the money earned did not accumulate for the final payment. At the end of the practice session, participants received feedback concerning their success ("You have accomplished the practice session with x out of 21 correct answers.").

Following the practice phase, participants experienced one of the nine experimental conditions (24 participants—12 males and 12 females—in each condition) that differed in the number of simulated partners per group (5, 10, or 15 partners) and the number of interactions with each partner (5, 10, or 15 interactions). To keep the number of trials as similar as possible for each participant, we replicated some of the conditions several times until the participants experienced between 150 and 225 trials. Thus, some conditions had only one replicate, whereas others had up to six replicates (Table 1.1).

Table 1.1

Experimental conditions.

Condition #	Partners	Interactions	Replicates	Total trials
1	5	5	6	150
2	5	10	3	150
3	5	15	2	150
4	10	5	3	150
5	10	10	2	200
6	10	15	1	150
7	15	5	2	150
8	15	10	1	150
9	15	15	1	225

Each replicate consisted of a series of rounds, each with a different set of partner names and images. Participants met with each partner once in a randomized order per round. In the first round, we presented individually for each partner a photograph, a name, and an action: for instance, "Ulrich cooperates" (Figure 1.1). All partners were male, and we randomized partner names and photos across participants. Participants viewed each partner's information for 5 s before advancing to the next partner (1 s in between partners). For every trial in the experimental phase, we randomly assigned the partner's action as cooperate or defect, so participants could not associate a pattern of action with each partner and had to track the exact behaviour of each partner in the previous round.

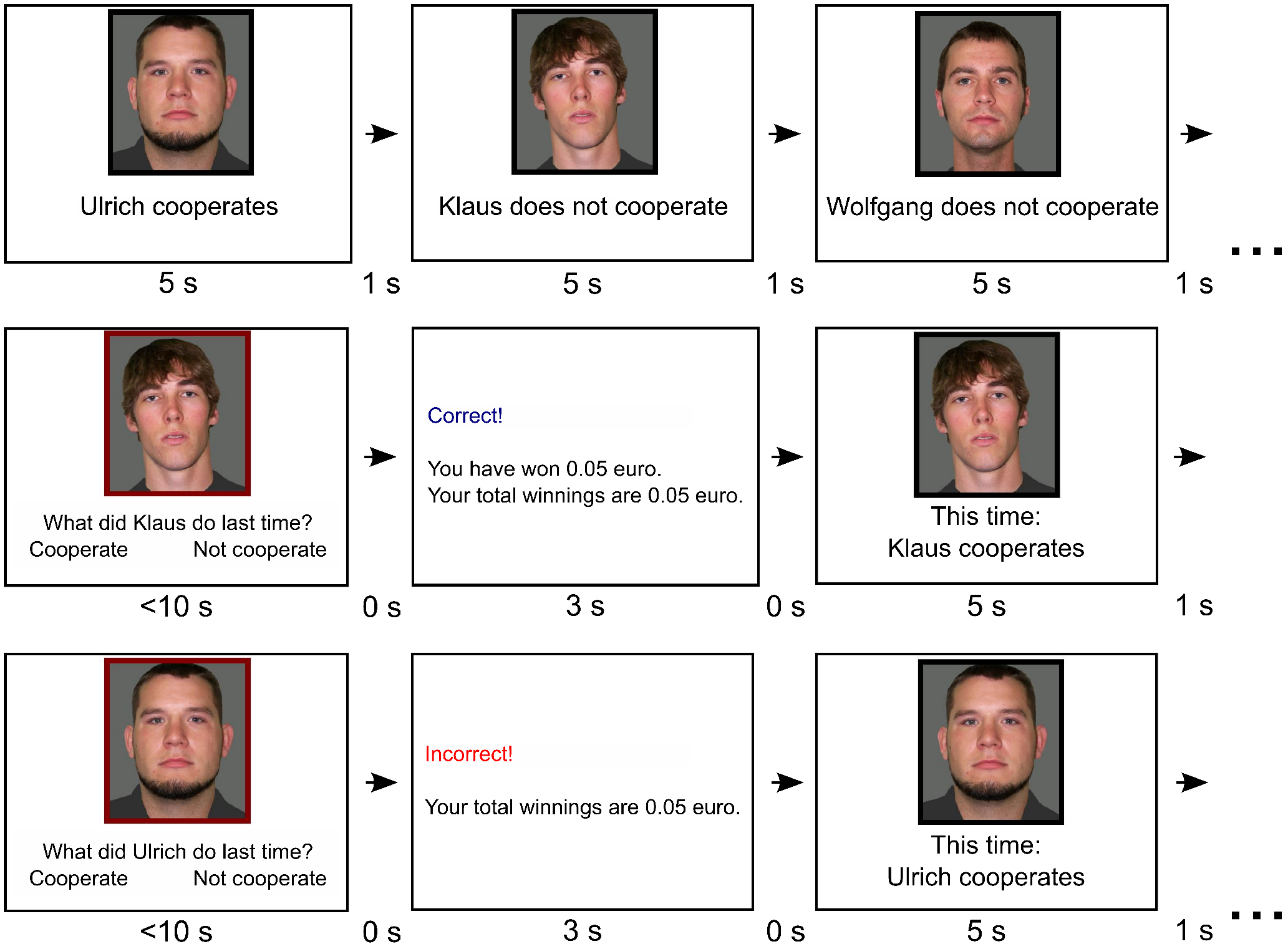

Figure 1.1. Screen shots of the cooperative memory task. In the first round of the task (top row), participants observed an image and the name of each partner, along with the current action. After viewing this for each partner, participants were asked for a partner's previous choice, given feedback on his or her response, and updated on the partner's new choice before moving on to the next partner (middle and bottom rows). Numbers below screens give presentation times for screens and between screens.

After viewing all members of one group, participants began the retrieval rounds, with a randomized order of partners in each round. We presented the image of the partners, along with the question "What did (name) do last time?". The participant had 10 s to answer by pressing "k" or "n" ["*kooperiert*" ("cooperate") or "*nicht kooperiert*" ("did not cooperate")] on the keyboard. If they responded within 10 s, they received a feedback screen for 3 s stating whether they were correct, the amount of money they received for that trial (only if they were correct), and an updated total amount received so far in the experiment. If they failed to respond in time, the participant did not receive feedback, only a reminder to respond more quickly next time. After the feedback screen, participants viewed the new action of the current partner for 5 s before advancing to the next partner. In between rounds, participants could pause the programme and start a new round at their discretion. Afterwards, participants completed a questionnaire asking what kinds of strategies they used to solve the memory task, as well as how often they guessed and how often they thought the partners cooperated (see Participant Questionnaire in Appendix Document A2). Participants received 0.05 Euro for each correct answer and 5 Euro for showing up, earning an average of 11.05 Euro (approximately 14 US dollars) per person (range = 8.25–14.60 Euro). We analysed the data using R statistical software version 2.12.0 (R Development Core Team, 2010) and the epicalc (Chongsuvivatwong, 2010), Hmisc (Harrell, 2010), and lattice (Sarkar, 2008) packages. The original document for this paper used Sweave (Leisch, 2002) to embed the R code into the document, thus ensuring reproducible research (de Leeuw, 2001).

For the photographs of partners, we used images from Ebner (2008) downloaded from the Center for Vital Longevity: http://vitallongevity.utdallas.edu/stimuli/facedb/categories/neutralized-faces-by-natalie-ebner.html. We used 9 images of females for the practice phase and 31 images of males for the experimental phase. The depicted persons ranged between 18 and 32 years old, with the same background and colour of clothing (Ebner, 2008). For partner names, we used 40 of the most common male German names from 1958 to 2000 (retrieved from http://www.gfds.de/vornamen/beliebteste-vornamen/).

Results

As shown in Figure 1.2, participants made more errors as group size increased. With a group size of 5 partners, participants made errors in a mean (± 95% confidence interval) of 9.5 ± 2.3% of trials, whereas with 10 and 15 partners, they made errors in 22.5 ± 2.5% and 24.0 ± 2.5% of trials respectively. Participants performed fairly accurately at the smallest group size, but once required to track 10 or more partners, memory errors increased dramatically. In fact, the error rates in the 10- and 15-partner conditions suggest that participants were guessing in half of the trials. Thus, retroactive interference from tracking multiple partners sharply increased memory errors in this task.

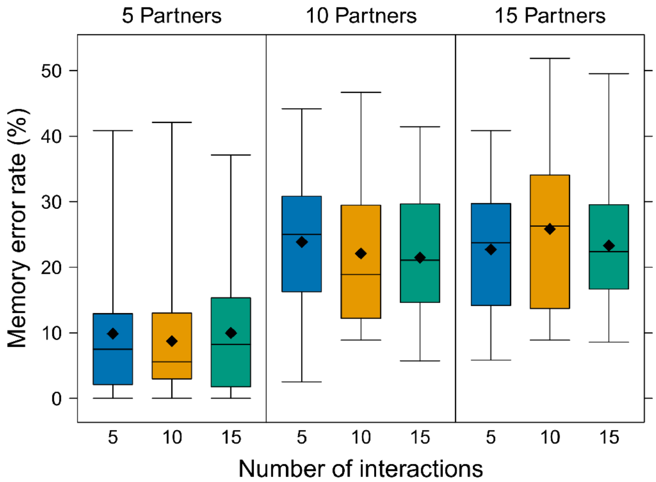

Figure 1.2. Memory error rate as a function of partner number and number of interactions. Boxplots show that the error rate increased with group size (N = 24 participants in each of nine conditions). The number of interactions per partner, however, did not influence error rate. Diamonds represent the mean, lines represent the median, boxes represent the interquartile range, and whiskers represent 1.5 times the interquartile range.

To further explore this memory interference, we examined error as a function of the number of intervening interactions. Between consecutive presentations of the same partner, there were other intervening partners. Because we randomized the order of presentation of partners within a round structure, we had variation in the number of intervening interactions and could test whether more intervening events resulted in worse memory performance. When consecutive interactions with the same partner occurred with no intervening interactions, participants performed well, with a mean error rate below 10% (Figure 1.3). With even one intervening interaction, however, error rates doubled. With more intervening events, errors continued to increase but at different levels for 5 partners compared to 10 and 15 partners.

With these data, we could estimate a function describing how forgetting increased with the number of intervening interactions. When combining the participants experiencing 10 and 15 partners, these data were well described by the power function $p = 1 - 92(1 + n)^{-0.08}$ ($R^2 = 0.90$), where p represents the probability of an error, and n represents the number of intervening interactions. A similar analysis on the 5-partner data yielded the power function $p = 1 - 96(1 + n)^{-0.04}$ ($R^2 = 0.90$). We used a modified version of Wickelgren's (1974) function, because it predicts memory data well (Wixted & Carpenter, 2007).

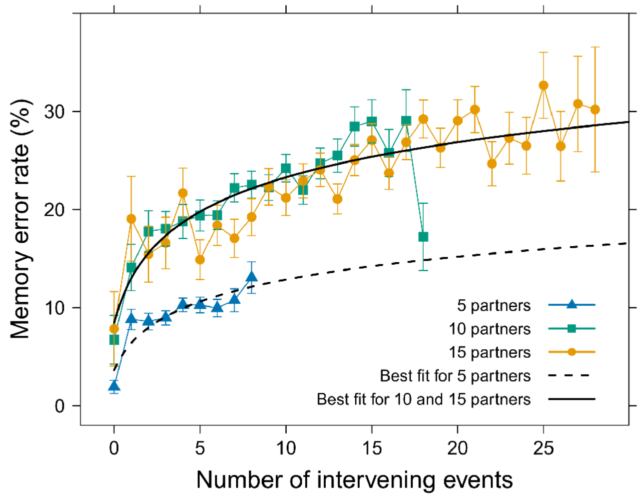

Figure 1.3. Interference effects on memory accuracy. The mean (± SEM) error rate increased with more intervening interactions across all three group sizes (collapsing across the number of interactions per partner), with the effect more pronounced in group sizes of 10 or 15. The smooth lines represent the least-squares best-fit Wickelgren's (1974) power function of memory to either the 5-partner data or combined 10- and 15-partner data (for both lines, $R^2 = 0.90$).

We also examined whether experiencing 5, 10, or 15 interactions with each partner influenced error rates. Surprisingly, the number of interactions did not influence performance (Figure 1.2). An examination of the trend in error rates across the course of the experimental session suggests a general learning effect. Participant errors increased in early rounds, indicating that more interactions caused more mistakes (Figure 1.4). Yet, in later rounds, performance almost returned to first-round levels, perhaps due to the participants' developing particular mnemonic strategies. In a questionnaire after the experiment, we asked participants to describe any strategies that they used during the cooperative memory task. A common strategy was to memorize either the cooperators or defectors and then infer the other. Also, participants frequently tried to focus on either positive (for cooperate) or negative (for defect) features of the faces or applied additional letters to the names (e.g., when Tim cooperates, remember Timk or Timko). Some elaborate strategies generated stories (e.g., "I eventually imagined that all the cooperating partners were members of my 'gang' and tried to talk myself into disliking the 'traitors'."). It appears as though participants used specific strategies to help in recall, which may account for the decrease in error rates over trials.

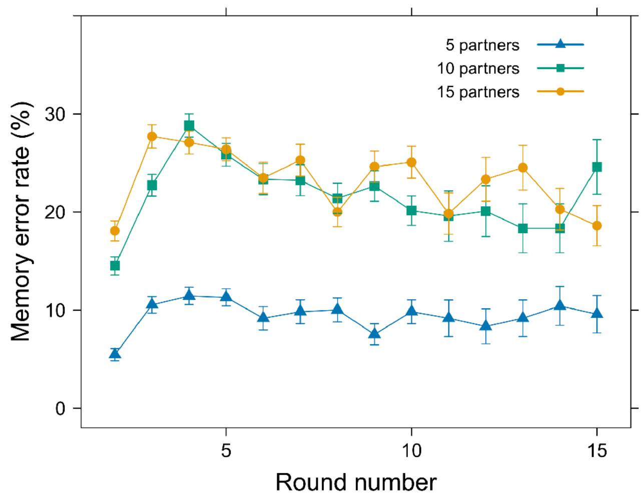

Figure 1.4. Error rate as a function of round number. The mean (± SEM) error rate increased in the first three or four rounds before decreasing.

Males and females did not differ in their error rates (males: 19.2 ± 0.6%; females: 18.8 ± 0.6%), and participants experienced similar error rates for cooperation and defection actions (cooperation: 19.2 ± 0.6%; defection: 18.8 ± 0.6%), suggesting no preferential memory for defectors or "cheaters" in this context (Cosmides & Tooby, 1989, Mealey, Daood & Krage, 1996).

Because both the images and names used as stimuli in this experiment varied in terms of attractiveness (Rudolph, Böhm & Lummer, 2007, Ebner, 2008), we examined the mean memory performance aggregated over all participants for the images and names that were rated for attractiveness. Attractiveness, however, did not correlate with memory performance for the images ($N = 40$, $r = -0.21$, $p = 0.19$) or names ($N = 19$, $r = -0.10$, $p = 0.69$).

Simulation analysis

Methods

We conducted a set of agent-based simulations in Pascal in which each agent interacted in a series of repeated Prisoner's Dilemma games (Table 1.2).

Table 1.2

Prisoner's Dilemma matrix.

		Against	
		Cooperate	Defect
Payoff to	Cooperate	R = 3	S = 0
	Defect	T = 5	P = 1

In the simulations, agents used one of nine strategies in the interactions (Table 1.3): all cooperate (AllC), all defect (AllD), cTFT, gTFT, Grim Trigger (Grim), Random, TFT, TF2T, and WSLS, also known as Pavlov. The population consisted of 11 agents of each strategy type, resulting in 99 total agents. Based on one of the conditions from the experiment, agents interacted with 10 randomly chosen partners for 10 interaction rounds. After completing all interactions, we summed the payoffs over all interactions for each agent in the population. To generate a new population, we ranked all agents by their total fitness and accumulated the total population fitness, starting at the lowest-ranked agent. We then randomly chose (with equal probability) one number from 0 to the accumulated population fitness. The strategy of the agent associated with that randomly chosen number was added to the next generation. We repeated this procedure (with replacement) until we populated the next generation with 99 agents. In 2% of the reproductive events, we randomly mutated the chosen strategy to one of the eight other strategies. We continued to produce new generations until all agents in a population played a single strategy. Simulations stopped when the entire population consisted of one strategy.

Table 1.3

Strategy descriptions.

Strategy	Description (with `computer implementation` and *game-theoretical definition*)
AllC (all cooperate)	Always cooperate. *Probability of cooperating following T, R, P, S = (1, 1, 1, 1)*
AllD (all defect)	Always defect. *Probability of cooperating following T, R, P, S = (0, 0, 0, 0)*

cTFT (contrite TFT)	Cooperate on the first move, then copy partner's choice on the previous move. If agent mistakenly defects, switch to cooperating.
	`if this is first interaction with partner, cooperate`
	`if partner cooperated on previous move, cooperate`
	`if partner defected on previous move & this is your`

	`second or third interaction with partner, defect`
	`if partner defected on previous move & this is your fourth or more interaction, look at own move before previous move:`
	`1) if you cooperated, defect`
	`2) if you defected, look at partner’s second previous move:`
	`a) if partner cooperated, cooperate`
	`b) if partner defected, defect`
	No game-theoretical definition—2-step memory strategy

Grim (Grim Trigger or Friedman)	Cooperate until partner defects, then always defect.
	`if this is first interaction with partner, cooperate`
	`if partner defected on previous move, defect`
	`if partner cooperated on previous move, look at own previous move:`
	`1) if you cooperated, cooperate`
	`2) if you defected, defect`
	Probability of cooperating following T, R, P, S = (0, 1, 0, 0)

gTFT (generous TFT)	Cooperate on the first move, then copy partner’s choice on previous move. If partner defected, cooperate with probability 0.33.
	`if this is first interaction with partner, cooperate`
	`if partner cooperated on previous move, cooperate`
	`if partner defected on previous move, defect with probability 0.66`
	Probability of cooperating following T, R, P, S = (1, 1, 0.33, 0.33)

Random	Randomly choose to cooperate or defect for each move. *Probability of cooperating following T, R, P, S = (0.5, 0.5, 0.5, 0.5)*

TFT (Tit-For-Tat)	Cooperate on the first move, then copy partner’s choice on previous move.
	`if this is first interaction with partner, cooperate`
	`if partner cooperated on previous move, cooperate`
	`if partner defected on previous move, defect`
	Probability of cooperating following T, R, P, S = (1, 1, 0, 0)

TF2T (Tit-For-Two-Tats)	Cooperate on the first two moves, then copy partner’s choice on previous move. If partner defected, look back another move, and if partner defected then, defect, otherwise cooperate.
	`if this is first interaction with partner, cooperate`
	`if partner cooperated on previous move, cooperate`
	`if partner defected on previous move & this is your second interaction with partner, cooperate`
	`if partner defected on previous move & this is your third or more interaction with partner, look at partner's second previous move:`
	`1) if partner cooperated, cooperate`
	`2) if partner defected, defect`
	No game-theoretical definition—2-step memory strategy

WSLS (Win-Stay, Lose-Shift or Pavlov)	Cooperate following mutual cooperation or mutual defection, otherwise defect.
	`if this is first interaction with partner, cooperate`
	`if you cooperated and partner cooperated, cooperate`

	`if you defected and partner defected, cooperate`
	`if you cooperated and partner defected, defect`
	`if you defected and partner cooperated, defect`
	Probability of cooperating following T, R, P, S = (0, 1, 1, 0)

We introduced error into the simulation by varying the probability of an agent "misremembering"—that is, remembering that the partner chose the opposite of what he actually chose—in six of the strategies: cTFT, Grim, gTFT, TFT, TF2T, and WSLS. For strategies using multiple previous actions from the partner (cTFT, TF2T), each memory had an independent probability of error.

No memory was necessary for AllC, AllD, and Random, and we assumed perfect memory for the agent's own action in cTFT and WSLS. We varied the error rate from 0 to 50% in 1% increments and conducted 1,000 simulations at each of the 51 increments. We report the proportion of the 1,000 replications in which each strategy dominated the population (i.e., the remaining strategy in the final generation).

Results

In the cooperative memory task, even when explicitly rewarded for recalling the last action of their partners, participants made mistakes in 10–24% of trials. Though these error rates seem quite high given that chance performance in this task is 50%, we need a criterion for determining whether decision strategies can maintain cooperation at the error rates demonstrated by our participants. To determine whether the existing decision strategies can cope with this level of error, we assessed how well these strategies performed when making mistakes in an agent-based simulation. Figure 1.5 shows for each error rate (1) the performance for each strategy (mean proportion of simulations in which each strategy outcompeted all other strategies) and (2) the proportion of interactions in the last generation in which the agents cooperated. At low error rates, Grim—a strategy that begins by cooperating, then permanently switches to defection following the partner's first defection—outperformed all other strategies. Though at odds with Axelrod and Hamilton's (1981) original results, this finding replicated results from Linster (1992) in which Grim dominated the populations in the absence of errors. Additionally, AllD, WSLS, TFT, and cTFT won a small percentage of the simulations. As error rates increased, AllD and Grim outcompeted TFT and the other cooperative strategies. The frequency of cooperative acts employed by all agents in the population decreased dramatically as errors became more prevalent. This decrease in cooperation reflected how the various strategies such as Grim switched from cooperating to defecting when memory errors increased. Thus, cooperation could not be sustained, even at low levels of error.

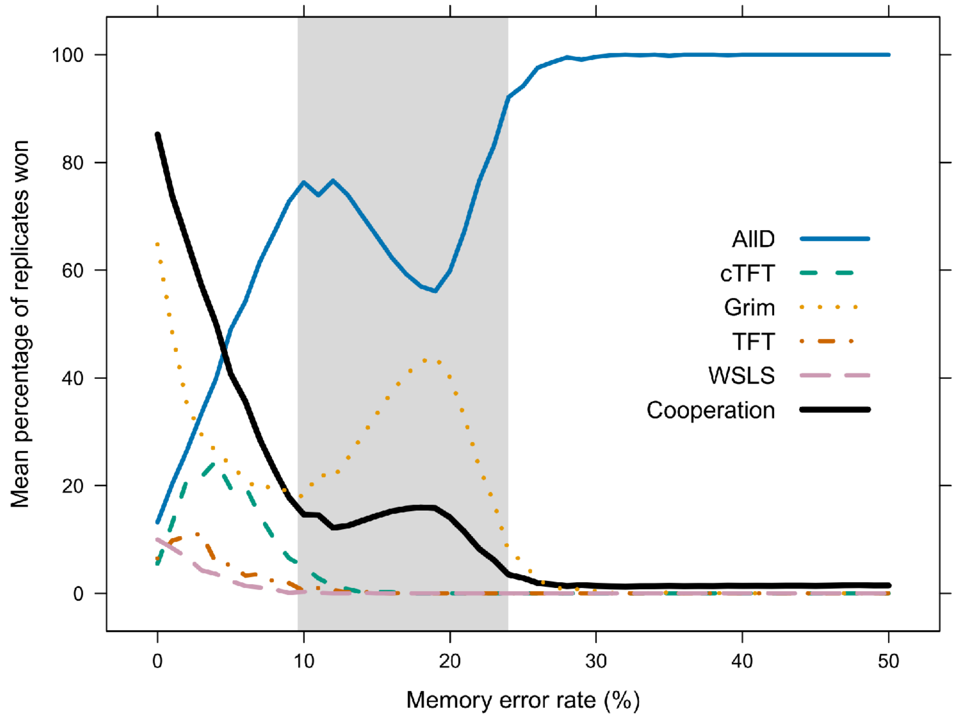

Figure 1.5. Agent-based simulations of error rate effects. When varying error rates across a range of values, Grim, cTFT, TFT, WSLS, and AllD survived with few errors (we do not show strategies with success rates lower than 0.05%). At higher rates (e.g., error rates observed in the experiment are shaded), however, AllD and Grim outperformed the other strategies. The proportion of cooperative choices made by all agents in the last generation decreased rapidly with increasing error rate.

To further assess the role of error on cooperation, we embedded the forgetting functions from our experimental data into the agents in our simulation. Instead of using a fixed error rate as in the previous simulation, we conducted a simulation in which the error rate depended on the number of intervening interactions, and we drew that error rate from the fitted forgetting function from the memory experiment. All other aspects of the simulation were the same as above, and we conducted 1,000 replications of this simulation.

Using this forgetting function to assign memory error as a function of number of intervening events yielded results similar to the fixed-rate analysis. AllD won around 83.0% of the simulations, whereas Grim won 17.0%, and only 6.2% of interactions involved cooperation. Even when using a lower-error forgetting function based on the 5-partner condition of the experiment, only strategies AllD and Grim performed well (winning 74.5 and 24.5% of the simulations, respectively), and we observed cooperation rates of 12.8%. The cooperative strategies that depend on memory of partners' last action failed when confronted with a realistic, forgetful memory.

Game-theoretical analysis

To verify our agent-based simulation results, we also used analytical methods to assess the role of error on cooperation by applying evolutionary game theory (Maynard Smith, 1982). Evolutionary game-theoretical analyses seek an evolutionarily stable strategy (ESS), that is, a strategy that, when adopted by all members of a population, cannot be outperformed (or invaded) by any alternative strategy. If a strategy A playing against itself has a higher payoff than any alternative strategy has against A [payoff(A, A) > payoff(alt, A)], that strategy A is an ESS. If the payoffs are the same, then A must have a higher payoff against the alternative strategy than the alternative strategy has against itself [payoff(A, alt) > payoff(alt, alt)] to be an ESS. Because we are interested in how error influences the payoffs of many strategies, we used Stephens et al.'s (1995) technique to calculate ESSes with error. This technique, however, only applies to strategies that use information from the single last interaction. Including earlier interactions greatly complicates the analysis, so we limited this analysis to the seven strategies that use only the last interaction: AllC, AllD, Grim, gTFT, Random, TFT, WSLS (Table 1.3). We used the standard Prisoner's Dilemma matrix (Table 1.2) and set the probability of future interaction to $\alpha = 0.9$ to approximate the 10 interactions used in our experiment. To estimate the payoffs for the remaining strategies (cTFT and TF2T), we used an agent-based simulation with two agents (one was either cTFT or TF2T and the other was one of the nine strategies) playing 10 interactions for 10,000 replicates. We calculated or simulated the payoffs to each strategy against each other strategy with error rates ranging from 0 to 50% in 1% increments.

We corroborated the simulation finding with a game-theoretical analysis. Figure 1.6 illustrates the game-theoretical payoffs of all strategies categorized by the strategy against which the others play (the "population" strategy). When the payoffs of a strategy playing against itself exceed the payoffs of all other strategies against it, the strategy is an ESS for these error rates. AllD was an ESS over the entire range of error rates. Grim was an ESS at error rates between 12 and 18%, validating its performance in the evolutionary simulation around that error rate (Figure 1.5). CTFT was an ESS at error rates between 0 and 17%, although these results are simulated and must be viewed with caution. Otherwise, none of the other strategies was evolutionarily stable for this range of parameters.

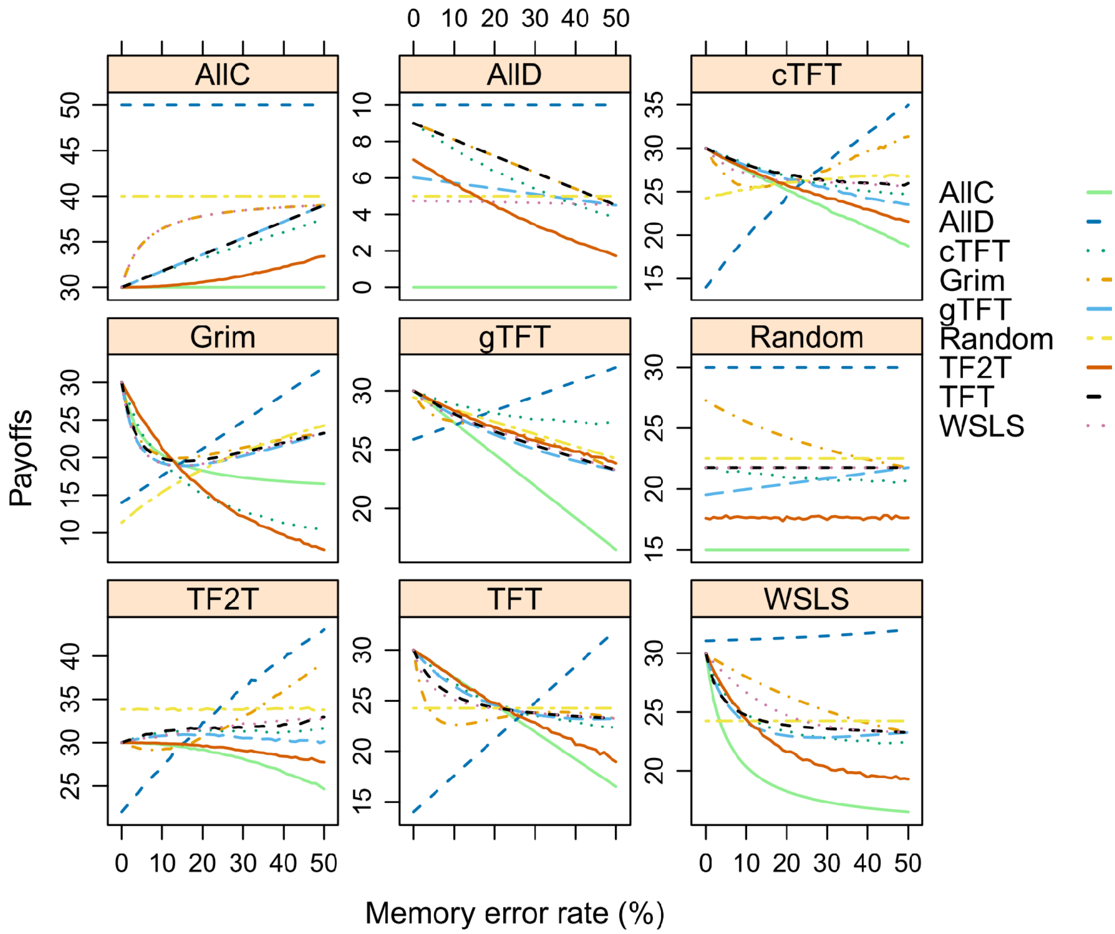

Figure 1.6. Game-theoretical payoffs of strategies as a function of error rate. For each strategy, we calculated how all strategies perform against that strategy over a range of error rates. When the strategy playing against itself has a higher payoff than any other strategy playing against it, this is an evolutionarily stable strategy (ESS). Strategies cTFT and TF2T were simulated rather than analytically calculated.

Discussion

The goal of this study was to test the psychological plausibility of the memory assumption implicitly embedded in models of decision strategies for repeated social interactions. These strategies assume that behaviour in a social interaction depends on the precise recall of a partner's past actions. We show that human participants have great difficulty accurately recalling the previous actions of simulated partners. Interference associated with tracking the behaviour of partners degrades memory performance, and having more partners results in worse performance. To assess whether the decision strategies proposed in the literature can sustain cooperation in the face of error, we conducted simulations of a repeated Prisoner's Dilemma game in which the agents sometimes forgot their partner's past actions. When mapping the experimental results onto the simulation results, we see that, in our simulation scenario, cooperation is not maintained, because few cooperative strategies perform well at the error rates shown by the experimental data. Instead, defection dominates with these estimates of error. These results held even when we used estimates of the best memory performance observed in our memory experiments. Of course, the results of the simulations are dependent on the strategies included and the parameters used. Nevertheless, these findings support the notion that a complete understanding of cooperation requires investigating the underlying cognition needed to implement those strategies (Stephens, McLinn & Stevens, 2002, Hammerstein, 2003, Stevens & Hauser, 2004, Stevens et al., 2005, Furlong & Opfer, 2009).

One limitation of our experiment is the artificial nature of the task, a limitation shared by most other cooperation experiments in psychology and economics. More realistic social interactions might trigger more effective memory performance, so we should pay careful attention to how cooperation evolves with error rates lower than what we observed. Though aspects of the task may be artificial, in some ways, our memory task actually underestimates error. For instance, we use rather small group sizes, ranging from 5 to 15 individuals. Estimates from Christmas card lists in England suggest average social network sizes around 125 individuals (Hill & Dunbar, 2003). Tracking the behaviour of this many individuals is quite daunting and likely would greatly increase the error rate. Additionally, our study minimizes the influence of events outside of the cooperative interactions on memory accuracy. In more realistic settings, many more aspects of real life may interfere with accurate memory. We asked participants to recall behaviour after rather short delays and with only a few intervening events. In our day-to-day lives, we constantly encode memories that may interfere with our ability to recall, with retention intervals extending into days, weeks, months, or even years between interactions. More realistic situations with larger numbers of social partners and longer time delays between interactions could actually make memory worse than that observed in our study. Thus, our task may be too difficult in some ways and too easy in others, but in either case, the strategies in question need to track behaviour with an exquisite memory.

Most empirical studies of the Prisoner's Dilemma involve repeated interactions with the same opponent. We created a more realistic situation by including multiple partners and interleaving interactions among partners (Winkler et al., 2008). A further improvement might be to offer a skewed interaction pattern. Rather than meeting all partners the same number of times, participants could have interacted more frequently with some partners than others, a pattern we observe in natural social encounters (Pachur, Schooler & Stevens, in press). These patterns of contact have interesting implications for cooperation, because the frequency of contact influences the expected time between contacts. Thus, retention intervals vary for tracking the previous behaviour of more versus less frequently contacted social partners.

Finally, in our task, we attempted to make the cooperation and defection events equally salient, but real cooperative interactions are much more heterogeneous: opening a door for someone will not be remembered in the same way as cheating on a spouse. The salience or magnitude of costs or benefits of the cooperative or defection event likely contributes to the retention of the memory (Mealey et al., 1996, Rankin & Eggimann, 2009). Yet, our analysis with lower-error rates (forgetting function based on the 5-partner condition) still showed minimal cooperation rates, indicating that better memory performance is not enough to sustain cooperation—near perfect memory is required. More importantly, we designed a task that is ecologically valid for TFT and the other decision strategies under investigation. These strategies do not invoke emotional salience or differential encoding of behaviour depending on the magnitude of costs or benefits. They all simply store a binary value (cooperate or defect) for each partner. Adding salience and magnitude effects means developing and testing new strategies, a path we fully endorse.

How might we circumvent the problem of memory in cooperation? Or, put another way, why do we see cooperation in Iterated Prisoner's Dilemma situations? There are at least two possibilities. The first is methodological. Many studies of the Prisoner's Dilemma have participants play against a single partner repeatedly. This may facilitate cooperation both because it provides much experience with a partner and because it limits the memory load associated with the more realistic scenario of tracking multiple partners. The second reason why we may see cooperation in these tasks is that people are using different strategies than those currently proposed in the literature. One possibility is a kind of meta-strategy in which people use TFT when they can remember past interactions and use another strategy when they cannot remember. Though this meta-strategy has not been investigated theoretically, people could use something like this to reciprocate. Alternatively, people may be using a longer-term reciprocal strategy. Instead of relying solely on the most recent behaviour when cooperating, they may build a reputation for partners, accounting for experience over several interactions (Roberts, 2008). People may implement reciprocal strategies that classify partners into types instead of track all individual choices. Though we focused here exclusively on direct experience with partners, people also likely use indirect experience by observing third-party interactions to build an image score for potential partners (Roberts, 2008, Rankin & Eggimann, 2009). Thus, instead of tracking individual interactions, people may encode more general summaries of behaviour, drawn from both personal experience and observing other interactions.

Rather than test how people actually make cooperative decisions, our intention here was to test whether the current decision strategies provide a suitable framework for exploring cooperation. We suggest that, though these models have proven valuable in investigating cooperation for the last 30 years, they do not accurately reflect underlying cognition. Humans certainly use reciprocal strategies when cooperating, but they likely do not use strategies like TFT and its relatives. Our results suggest that they simply cannot use these strategies, because the memory load is too great. To examine the types of reciprocal strategies that humans and other animals use, we must embed what we know about memory into new realistic cooperative strategies. Building psychology into these models is a crucial next step in better understanding the nature of cooperation.

Chapter 2
The Good, the Bad, and the Rare: Memory for Partners in Social Interactions

Preliminary note

Chapter 2 is based on the paper with the same title that resulted from collaboration with Jörg Rieskamp and Jeffrey R. Stevens (Volstorf, Rieskamp & Stevens, 2011). To comply with the rest of the thesis, I changed American into British English and adapted the numbering of tables and figures to be chapter specific. Available with the original paper are the participants' data, but because these would fill almost 20 pages, I omit them here and refer the interested reader to the source at http://www.plosone.org/article/info%3Adoi%2F10.1371%2Fjournal.pone.0018945.

In helping to make this paper what it is, we would especially like to thank Michaela Riediger for letting us access the FACES database and Sebastian Scholz for the additional pictures, Gregor Caregnato for testing the participants, Henrik Olsson, Nadine Fleischhut, Ana Sofia Morais, and the ABC research group for helpful comments, Pat Barclay for his kind correspondence and making the reanalysis of his data convenient, Edgar Erdfelder for his patient and thorough support with the multinomial processing tree model, Axel Buchner and Raoul Bell for sharing their submitted manuscript and the results on the reanalysis of Barclay's data, and two anonymous reviewers for their helpful comments.

Abstract

For cooperation to evolve via direct reciprocity, individuals must track their partners' behaviour to avoid exploitation. With increasing size of the interaction group, however, memory becomes error prone. To decrease memory effort, people could categorize partners into types, distinguishing cooperators and cheaters. We explored two ways in which people might preferentially track one partner type: remember cheaters or remember the rare type in the population. We assigned participants to either of three interaction groups which differed in the proportion of computer partners' types (defectors rare, equal proportion, or cooperators rare). We extended research on both hypotheses in two ways. First, participants experienced their partners repeatedly by interacting in Prisoner's Dilemma games. Second, we tested categorization of partners as cooperators or defectors in memory tests after a short and long retention interval (10 min and 1 week). Participants remembered rare partner types better than they remembered common ones at both retention intervals. We propose that the flexibility of responding to the environment suggests an ecologically rational memory strategy in social interactions.

Introduction

Which do you remember better, an interaction partner who treated you nicely or one who harmed you? Here, we investigated which kind of partner type people remember preferentially, the “good” or the “bad”.

Humans cooperate in a variety of contexts (e.g., Henrich & Henrich, 2007), although cooperators risk exploitation by cheaters’ accepting but not repaying the beneficial act. One mechanism proposed to explain cooperation between genetically unrelated individuals is reciprocal dependence in repeated interactions: For a cheater who will meet the exploited partner again, the costs of future withheld cooperation by that partner may outweigh the benefits of the current exploitation (Axelrod & Hamilton, 1981, Trivers, 1971). A prerequisite for this direct reciprocity is to identify each partner and remember the history of interactions—an error-prone memory would invite partners to cheat.

Although memory of the partners’ behaviour is an important building block for the emergence of cooperation (Stevens et al., 2005), storing all actions of all partners is not feasible for the boundedly rational human mind (Simon, 1956). Time (i.e., the delay until the next access to the information in memory) causes information traces to decay (Wixted & Ebbesen, 1991), and new and existing knowledge interferes with accurate recall. A study by Stevens, Volstorf, Schooler, and Rieskamp (2011) showed that even tracking the single last action of each interaction partner, as many of the proposed reciprocal strategies such as Tit-For-Tat demand (Axelrod, 1984), led to high memory error rates. In an evolutionary simulation, these errors resulted in a sharp reduction in cooperation. So, remembering either the complete interaction history or the single last action of each partner seem to be ruled out as potential candidates for the memory processes underlying cooperation. An alternative strategy could be to categorize partners into types reflecting their general behaviour, for example “cooperator” and “defector”, and remember these types. Compared to constantly updating each partners’ actions, the type is a more stable criterion and, therefore, decreases memory effort. Although categorizing partner types may ease memory requirements, the information on partner types is susceptible to forgetting, too. Here, we investigated two hypotheses, the “cheater-memory” and the “rarity” hypothesis, that both propose to remember one partner type preferentially and infer the other, thereby reducing memory load. Barclay (2008) and Bell, Buchner, and Musch (2010) also addressed these hypotheses, and we extended their approaches by giving participants repeated experience with their partners and testing memory after both a short and long retention interval.

Remember cheaters

One hypothesis predicts that, to reduce fitness costs associated with exploitation, individuals will remember cheaters preferentially. According to error management theory (Haselton & Buss, 2000), exploitation by a defector is worse than missing out on a cooperative opportunity. To prevent exploitation, individuals would not only benefit from detecting cheaters (Cosmides & Tooby, 1989), but because more important information has priority in memory than less important one (Schulz, 1971), they would also benefit from remembering cheaters preferentially (Mealey et al., 1996). We term this the cheater-memory strategy.

In an environment with a majority of cooperators, preferentially remembering the few cheaters reduces the probability of memory errors and related costs. Some environments, however, may contain a majority of cheaters and here, adhering to the cheater-memory strategy would not reduce memory load much.

Remember the rare type

The second hypothesis emphasizes the costs of memory rather than the costs of exploitation. Individuals cope with a variety of environments, and, so, rather than always remember cheaters, they might benefit from a memory strategy that adapts to different contexts. Such an ecologically rational (Todd & Gigerenzer, 2007) strategy would preferentially remember the less frequent partner type (Barclay, 2008). This does not just reduce the amount of information to retain but also potential memory errors. We term this the rarity strategy.

In addition to reducing memory load, focusing on the rare type could be beneficial, because it is novel and striking. Since 1933 (von Restorff, 1933), researchers have investigated why people better remember distinctive events. The reason, according to Hunt (2006), is not an objects' property but the objects' processing via increased attention and memory. Schmidt (1991) proposed the incongruity hypothesis, which provides a combination of property- and process-explanations and allows adaptation to the environment. Given this definition, one partner type may be preferentially remembered in one context (i.e., an interaction group where it is in the minority) but not in another (i.e., an interaction group where it is in the majority).

Testing cheater-memory and rarity strategies

With this study, we explored two hypotheses regarding which partner type people remember preferentially.

1. According to the cheater-memory strategy, cheaters are remembered better than cooperators, regardless of the number of cheaters or cooperators in the environment.
2. According to the rarity strategy, the rare partner type in an environment is remembered better than the common one; for example, people remember cheaters better only when these are in the minority in the environment.

Adhering to the basic procedure of partner-type memory studies since the seminal paper by Mealey et al. (1996), we evaluated participants' memory of the partners by mixing the faces we had presented to participants, the old faces, with new faces. Then, for each face, we asked whether participants had seen it in the beginning of the experiment (*recognition*; e.g., Mealey et al., 1996, Barclay & Lalumière, 2006, Chiappe et al., 2004). Incorporating Mehl and Buchner's (2008) suggestion that recognition alone cannot be evolutionarily beneficial, because it does not allow a sufficient partner identification, we additionally asked whether the partner was a defector or cooperator (*categorization*). To test the hypotheses, we varied the proportion of partner types in the interaction group. This has also been done by Barclay (2008) and Bell et al. (2010) who each found support for a rarity strategy. The design of both studies, however, left open two questions that we addressed here.

Does experience influence categorization?

To indicate that partners are cooperators or defectors, some researchers provided each partner's face with a description of cooperative or non-cooperative behaviour (e.g., Mealey et al., 1996), Chiappe at al., 2004, Farrelly & Turnbull, 2009). Barclay and Lalumière (2006) criticized these descriptions, as participants could perceive the degree of cheating as higher as that of cooperation, which could lead to a stronger encoding of the cheaters. Moreover, we believe that behaviour descriptions likely do not have a large enough impact on participants' behaviour and memory (see Gigerenzer & Hug, 1992 concerning the importance of the perspective on the cheater-detection mechanism in social-contract violations). In contrast, testing partner-type memory using an economic game (e.g., Oda, 1997, Singer, Kiebel, Winston, Dolan & Frith, 2004) has two advantages. First, games avoid uncertainties about the degree of cooperation and defection. In a Prisoner's Dilemma game, for example, cooperation and defection are not indicated by example descriptions but one of two choices (cooperate/defect) the partner takes, and these choices are associated with a payoff matrix. Second, participants experience the consequences of their partners' behaviour directly. Rather than having participants evaluate whether partners with little relation to their welfare have violated social contract or hazard management rules, using a game affects participants immediately, because the payoff depends on their own and the partner's decision. Compared to pure behaviour descriptions, the strategic nature of the Prisoner's Dilemma likely triggers behaviour and memory processes for tracking cooperators and defectors.

Barclay (2008) and Bell et al. (2010) employed economic games but in a limited way. Barclay (2008) only announced to participants what their partners would do in a trust game that followed the memory test. Bell et al. (2010) emphasized the importance of personal involvement for partner-type memory and let participants experience their partners in a trust game, but it was one-shot and gave participants just a single instance of the type of partner they were facing. We believe it is more realistic if participants are not just confronted with a label or a one-time experience but meet their partners repeatedly (Hertwig & Ortmann, 2001). This enables participants to infer the partners' types on their own and increases the recognition accuracy, as people remember self-generated items better than ready-made ones (Slamecka & Graf, 1978). Repeated interactions also mimic situations outside the lab in which remembering with whom to cooperate and with whom not to cooperate is the prerequisite for establishing reciprocal relationships.

How robust are the memory strategies to longer retention intervals?

The majority of studies on partner-type memory tested recognition (and categorization) in a memory test after either several minutes (Barclay, 2008, Chiappe et al., 2004, Farrelly & Turnbull, 2009, Singer et al., 2004, Bell & Buchner, 2009) or 1 week (Mealey et al., 1996, Barclay & Lalumière, 2006, Oda, 1997) following the initial presentation of the partners by mixing the familiar with new partners. We investigated whether the memory effort associated with longer retention intervals influences the memory strategies. Thus, we asked participants for recognition and categorization of partners after retention intervals of both 10 min and 1 week. Though others have tested the effect of a short and long retention interval in cheater-memory studies (Mehl & Buchner, 2008, Buchner, Bell, Mehl & Musch, 2009), no one has done so for the rarity strategy.

In sum, to test the cheater-memory versus rarity hypothesis, we had participants experience their computer partners' types in repeated Prisoner's Dilemma games. We varied the proportion of defectors and (conditional) cooperators among partners in a between-subjects design. Then, participants answered recognition and categorization questions in a memory test after 10 minutes and again after 1 week.

Methods

Ethics Statement

The ethics committee of the Max Planck Institute for Human Development approved the study. Participants signed an informed consent before proceeding with the experiment.

Participants

Our lab recruited 126 participants (63 males, 63 females; mean [*M*] age = 26, range = 18–37, median [*Mdn*] = 26, mode [*Mo*] = 25) from the Berlin universities, 97 of which were students or in training. We excluded one participant for the analysis of the second session due to technical problems.

Stimuli and materials

For interaction partners, our design required 68 images of males and females with neutral facial expressions (participants and depicted volunteers were roughly of the same age). We used 58 colour portraits from the database FACES (Ebner, Riediger & Lindenberger, 2010; http://faces.mpib-berlin.mpg.de/album/escidoc:57488), with the volunteers wearing grey shirts, no make up or jewellery, sitting in front of a dark-grey background. For the remaining 10 images, we photographed students at the Technical University of Chemnitz in the same way as the FACES portraits. We randomly assigned popular German names (from the website http://www.beliebte-vornamen.de/) to the images for each participant. To avoid confusion about and interpretation of the faces, we informed the participants about the neutral character of the images.

We programmed and presented the experiment with E-Prime experimental software (Schneider et al., 2002a, Schneider, Eschmann & Zuccolotto, 2002b) (programme available upon request). Participants received a written copy of the instructions during the experiment. The instructions contained the procedure of the interactions, illustrated with screen shots from the programme (Appendix Document B1; original German instructions available upon request). In explaining the interactions in the instructions, we neither mentioned the Prisoner's Dilemma nor the words *game*, *payoff*, or *player*. Instead, we instructed participants that the aim of the experiment was to engage in a social interaction with a partner with whom they cannot correspond. An example illustrated this. The participants did not know about the memory task beforehand. As the final task, participants completed a questionnaire (Appendix Document B2) concerning their possible strategies and other remarks.

Procedure

The experiment involved two sessions separated by a mean of 7 days (range = 5–10 days, *Mdn* = 7, *Mo* = 7). The first session consisted of five phases and took approximately 80 min; the second session comprised four phases and lasted about 40 min.

Session 1

After the participants had read the written instructions, we tested their understanding of the payoff matrix (Table 2.1) in the first phase of the session. We presented them with the four possible game outcomes (for example: “I cooperate, the partner refuses to cooperate.”) and asked them to indicate each time how many points they and their partner would receive according to the payoff matrix. They had to answer all situations correctly to continue to the next phase; otherwise, they repeated the phase (90 participants answered all four questions correctly after one round, 34 after two rounds, one after three rounds, one after six rounds).

Table 2.1

The Payoff for the Prisoner’s Dilemma game.

Player’s Choice	Partner’s Choice	
	Cooperate	Refuse
Cooperate	3 ; 3	0 ; 5
Refuse	5 ; 0	1 ; 1

Note. Payoff on the left in each cell is paid to the player, on the right to the partner.

In the second phase, participants practised the interaction task by experiencing a series of Prisoner’s Dilemma games in which they chose to cooperate or defect with each partner. The accumulated points, however, did not contribute to their final payment. Participants experienced four interaction partners whom they met for three interactions each (i.e., 12 encounters). Of the interaction partners, two were defectors (one male, one female) and two were cooperators (one male, one female). Afterwards, participants received feedback about their success ("You accomplished the practice session with [number] points profit. It would have been possible to achieve 14 to 24 points.") and had the possibility for a short break.

The third phase was the actual interaction task in which we converted the payoff participants received into money. We randomly assigned participants to three conditions (42 participants in each condition) that differed in the proportion of partner types among the 20 computer partners. In the "defectors-rare" condition, 20% of interaction partners defected, 80% cooperated. In the "equal-proportion" condition, 50% defected, 50% cooperated, and in the "cooperators-rare" condition, 80% of partners defected and 20% cooperated. Each type comprised half male and half female partners. Whereas defector partners always defected, cooperator partners played Tit-For-Tat, which starts by cooperating and then copies the participant's previous action. Implementing a strategy that reacts to the participants' behaviour maintains attention to the partner type. If participants faced a purely cooperative strategy, they might defect throughout to receive the highest payoff, losing the motivation to distinguish between the partner types. We informed participants that the partners were not human players but pursued strategies that had been identified in humans in experimental contexts before. They knew about neither the number nor nature of the partners' strategies.

In the first block of interactions, participants met each of their 20 partners once. This procedure was repeated for 10 blocks, with a random order each time. Each encounter began with the presentation of the partner (Figure 2.1). After 4 s, the next screen asked participants to either cooperate or refuse to cooperate with the partner and showed a picture of the payoff matrix. Participants had 10 s to respond; otherwise, this interaction was skipped, and they were asked to answer more quickly next time. The subsequent screens displayed the decision of the partner for 3 s and finally gave a summary of the interaction for 2 s.

After a distraction task in which participants completed a shortened version of an episodic memory task (Shing, Werkle-Bergner, Li & Lindenberg, 2008, Pachur, Mata & Schooler, 2009) in 10–15 min, the final phase of the first session was the memory task (Figure 2.1). Here, participants saw images of the 20 old partners mixed with 20 new ones (half males, half females) and, for each partner, had to answer three questions. The first screen presented the partner for 4 s. Then, participants had 5 s to decide whether they had seen the partner before (recognition). They did not receive feedback on their success. Second, they rated the cooperativeness of the current partner on a scale from 0 (*no cooperative actions*) to 100 (*always* *cooperative*) and, third, categorized him or her as a defector or cooperator. On the latter two questions there was no time limit. Participants repeated this memory task for each partner.

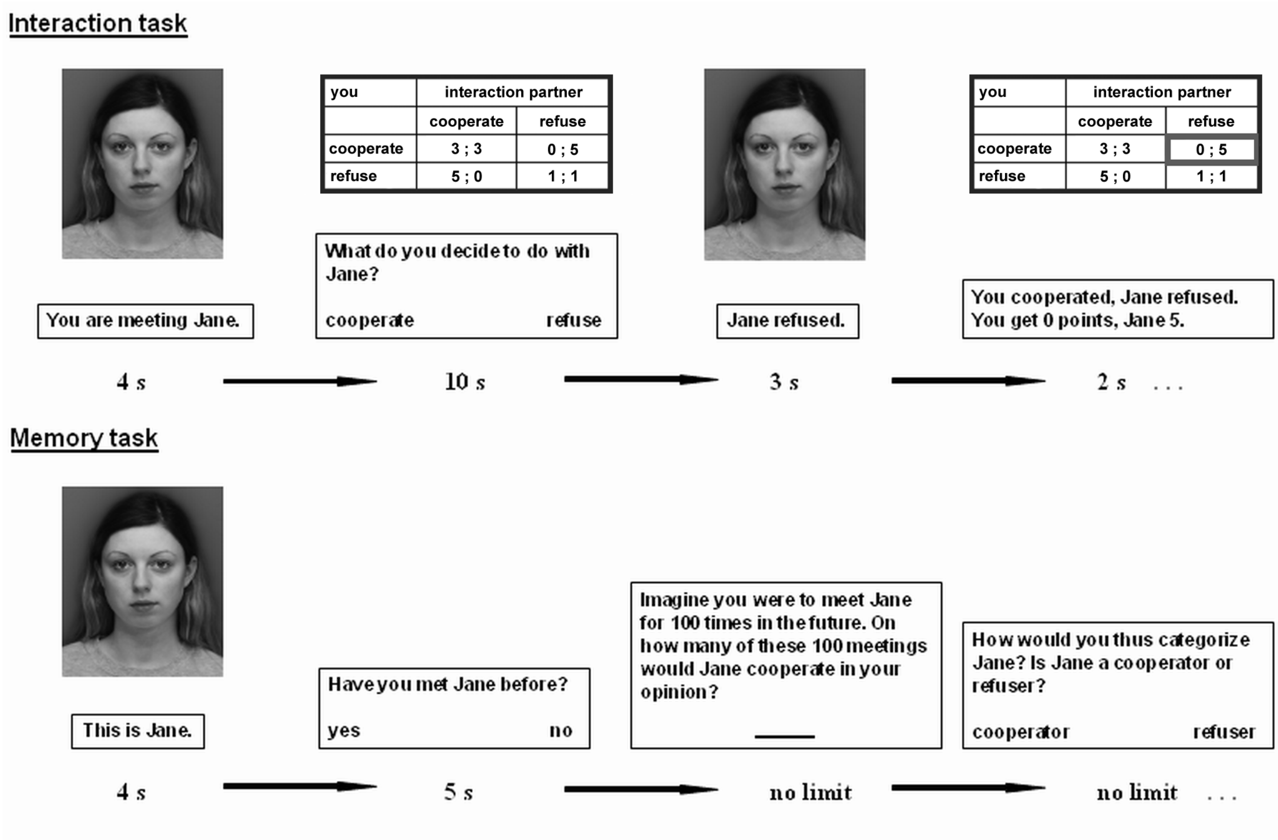

Figure 2.1. Example procedure of the interaction and the memory task. Screen presentation times are noted below. The original pictures were in colour.

Session 2

After a mean of 7 days, participants returned for the second session. They began by reading the written instructions and then proceeded with the memory task. We presented participants with the 20 old partners from the first experimental session and 20 new partners (half males, half females) they had not seen in any phase before. The procedure of the memory task was the same as in the first session.

Afterwards, participants had the opportunity for a short break and then experienced three Prisoner's Dilemma games with 20 partners from the memory task. Half of these partners were old, the other half participants had not seen before. The proportion of types among the partners conformed to the conditions like in the first session, and, again, each type comprised half male and half female partners. The procedure of the interactions was the same as in Session 1. Then, participants completed the distraction task, and, in the final phase, they answered questions concerning their strategies in the two sessions. Finally, participants received 5 Euro show-up fee per session. Additionally, we paid participants according to the overall points received in the interaction task in both sessions by multiplying their gains with 0.02, 0.03, and 0.06 Euro in the defectors-rare, equal-proportion, and cooperators-rare condition to equate the total payment across conditions ($\text{range}_{\text{Defectors Rare}}$ = 7.98–13.40 Euro; $\text{range}_{\text{Equal Proportion}}$ = 10.11–13.86 Euro; $\text{range}_{\text{Cooperators Rare}}$ = 9.36–19.56 Euro). Although participants received different numbers of points due to the proportion of partner types in the conditions, this did not influence the absolute number of partners correctly recognized and categorized. Participants did not know about the different exchange rates when making their choices and categorizations, so this could not influence the results.

Design and data analysis

As the independent variable, we varied the proportion of defectors and cooperators in the interaction group in a between-subjects design (defectors rare, equal proportion, cooperators rare). As the main dependent variables, we assessed recognition and categorization judgements, as well as a quantitative cooperativeness evaluation for each partner. Moreover, we collected choice data—the participants' proportions of defection against and cooperation with partners—to check whether participants were able to distinguish between the partner types. With descriptive statistics, we present mean, standard deviation, median, and mode; for comparisons between proportions, we give the mean with 95% confidence interval (e.g., Cumming, Fidler & Vaux, 2007) and Cohen's (1977) h effect size (Cohen's conventions: small effect size: $h = 0.20$, medium effect size: $h = 0.50$, large effect size: $h = 0.80$). If the proportions are compared to chance performance at 50% (i.e., 0.5), we report Cohen's g (Cohen's conventions: small effect size: $g = 0.05$, medium effect size: $g = 0.15$, large effect size: $g = 0.25$). When comparing results between sessions or repetitions, we accounted for within-subject variation by applying Morey's (2008) correction of Cousineau's (2005) transformation for confidence intervals. To evaluate the recognition performance, we provide Snodgrass-Corwin-corrected d' measurements (e.g., Schooler & Shiffrin, 2005).

We looked for the cheater-memory and rarity strategy in the categorization accuracy in conjunction with correct recognition of old partners, because also in everyday life one must do both—correctly recognize and correctly categorize to sufficiently identify a partner. In the memory research literature, this measure is called SIM, source identification measure (Bröder & Meiser, 2007), and is calculated as the number of correct categorizations given correct recognition over the number of all answers (correct recognition and correct categorization, correct recognition and incorrect categorization, incorrect recognition) for each partner type. Because participants make errors and guess when categorizing partners, the data analysis should account for guessing biases (Barclay, 2008, Bell et al., 2010, Bell & Buchner, 2009, Buchner et al., 2009). We calculated chance levels, that is, the accuracy achieved by guessing, as the proportion of categorized defectors and cooperators any time participants recognized a partner, whether old or new, as old and corrected the raw accuracy rates for these chance levels.

Results

Exclusion of participants

Participants who almost never cooperated experienced only minimal cooperation by Tit-For-Tat partners and, thus, could hardly distinguish between the partner types. From these participants, we did not expect proper partner identification in the memory task. We excluded 27 participants (four, 11, and 12 participants from the three conditions) who cooperated with Tit-For-Tat partners in at most 13% of the cases. At this percentage, there seemed to be a natural gap in the data. The next nearest value of "percentage of cooperation with cooperator partners" in the defectors-rare, equal-proportion, and cooperators-rare condition was at 18%, 27%, and 20%. All further analyses, therefore, used only the data from the remaining 99 participants. By excluding 27 participants, the mean proportion of cooperation with defector and cooperator partners increased, specifically for the equal-proportion and cooperators-rare condition. Moreover, the mean cooperativeness evaluation of cooperator partners increased. The categorization accuracy increased, specifically for the equal-proportion and cooperators-rare condition. All in all, however, by excluding the 27 participants, the results did not change dramatically. Additionally, we excluded cooperativeness evaluations from one participant in the defectors-rare condition in both sessions who seemed to have misunderstood the task and evaluated partners categorized as defectors with values around 96.2 (SD = 7.4) and partners categorized as cooperators with values of 1 (SD = 0).

Cooperative behaviour

Session 1

Participants experienced 10 Prisoner's Dilemma interactions with each partner. To maximize their payoff (Table 2.1), participants should defect against a defector partner and cooperate with a cooperator partner. Whereas participants cooperated with defectors less than expected by chance in all conditions, they cooperated with cooperators more than expected by chance only when cooperators were common (in the defectors-rare condition; *g* = 0.08; Figure 2.2) and when both partner types had an equal proportion (*g* = 0.05). Yet, we found more cooperation with cooperators than defectors in all conditions, and participants cooperated more with cooperators when they were common (in the defectors-rare condition; *M* = 58.0% ± 7.7 CI) than when they were rare (*M* = 48.5% ± 7.4 CI; h = 0.20; no difference between the other conditions).

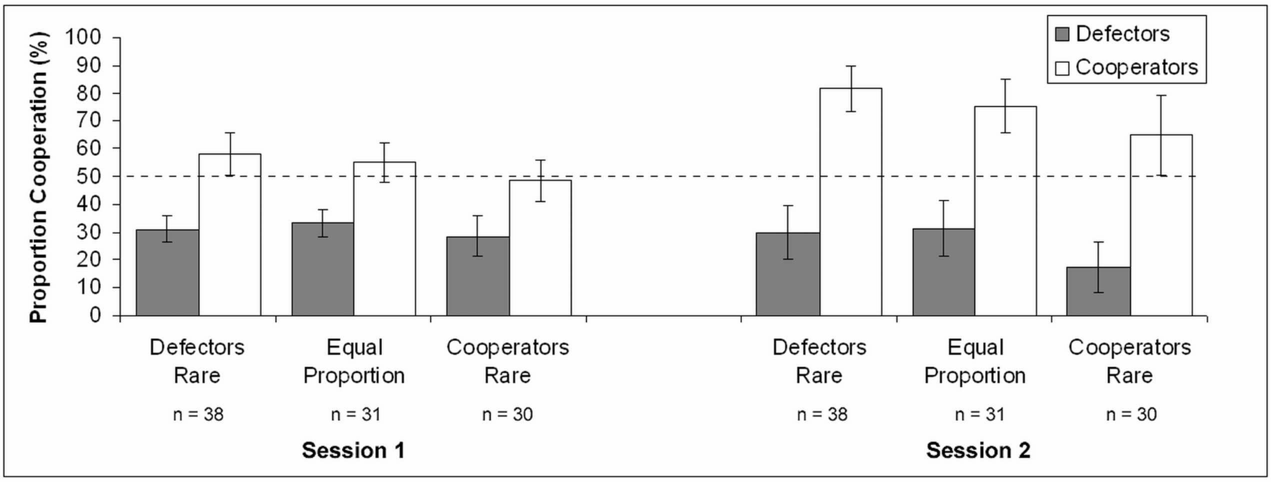

Figure 2.2. Proportion of cooperation with defector and cooperator partners. The dashed line represents chance performance. In the defectors-rare condition, of the 20 interaction partners 20% were defectors and 80% cooperators (Tit-For-Tat). The equal-proportion condition included 50% defectors and 50% cooperators, and the cooperators-rare condition included 80% defectors and 20% cooperators. The *n*s give the number of participants per partner type. Error bars represent 95% confidence intervals.

We did not expect these low rates of cooperation with cooperator partners, but the analysis across the 10 repetitions revealed that the mean cooperative behaviour increased from 40.8% ±

5.8 CI in the first round to 71.0% ± 4.3 CI in the tenth round (Figure 2.3).

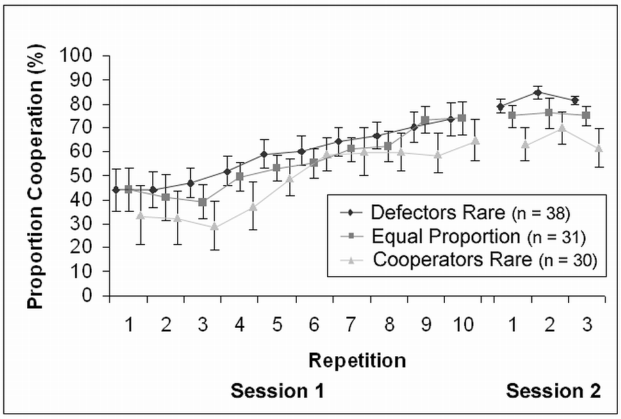

Figure 2.3. Proportion of cooperation across repetitions. The figure shows the mean proportion of cooperation (± 95% confidence intervals) with cooperator partners across the repeated interactions in the first and second session. Confidence intervals are corrected to account for within-subject variation (Morey, 2008).

Session 2

Figure 2.2 shows participants' cooperative behaviour, averaged across the three interactions, in the second session after about 1 week. This time, participants cooperated both with defectors less than expected by chance and with cooperators more than expected by chance in all conditions ($g_{\text{Defectors Rare}} = 0.32$, $g_{\text{Equal Proportion}} = 0.25$, $g_{\text{Cooperators Rare}} = 0.15$). They seemed to distinguish between the partner types and acted accordingly. Consequently, the proportion of cooperation with cooperators in the beginning of the second session (*M* = 72.9% ± 3.2 CI) was at the level of the tenth repetition in the first session (*M* = 71.0% ± 4.3 CI; Figure 2.3).

Recognition

Session 1

In the memory task, we first asked participants whether they had already interacted with each of 40 partners (20 old, 20 new ones). Participants recognized the 20 old partners accurately ($M_{\text{hit rate}}$ = 99.1%, *SD* = 2.5, *Mdn* = 100, *Mo* = 100) and showed low false alarm rates (false alarms / [false alarms + correct rejections]; $M_{\text{false alarm rate}}$ = 0.6%, *SD* = 1.7, *Mdn* = 0, *Mo* = 0; d' = 3.8)—they distinguished between old and new partners quite well.

Session 2

In the second session, participants showed high accuracy ($M_{\text{hit rate}}$ = 98.3%, *SD* = 5.1, *Mdn* = 100, *Mo* = 100) and low false alarm rates ($M_{\text{false alarm rate}}$ = 0.8%, *SD* = 3.7, *Mdn* = 0, *Mo* = 0; *d*` = 3.6), suggesting excellent recognition even after a one-week retention interval.

Cooperativeness evaluation

The second question of the memory task evaluated the cooperativeness of partners on a scale from 0 (*no cooperative actions*) to 100 (*always cooperative*). Overall, defector partners among the old partners received low cooperativeness values and cooperator partners received high cooperativeness values in both sessions, matching the strategies of the partner types (Figure 2.4). The larger variability for cooperator partners in each session reflects the reactivity of the Tit-For-Tat strategy to the participants' behaviour (cooperator partners' $M_{\text{cooperation rate}}$ = 57.2%, *SD* = 19.9, *Mdn* = 55, *Mo* = 50).

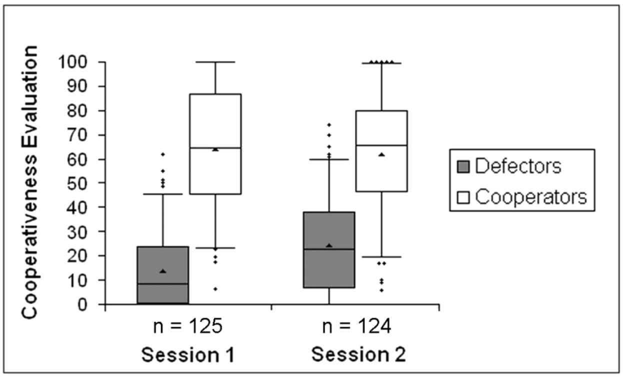

Figure 2.4. Cooperativeness evaluations of defector and cooperator partners. Boxplots represent cooperativeness evaluations of the partner types among old partners for both sessions. Boxplots show the median as a line inside the box, which contains 50% of the data (upper border = 75th percentile, lower border = 25th percentile). The triangle represents the mean. The whiskers range from 5 to 95% of the data, outliers are represented as diamonds. We additionally excluded the data from one participant in the defectors-rare condition in both sessions who seemed to have misunderstood the task.

Categorization

Session 1

We analysed the results of the memory task as the accuracy of categorization in conjunction with correct recognition of old partners. This accuracy rate, however, must be corrected for the chance level of accuracy reached by participants' guessing the answers. To represent the chance level, we considered the perceived proportion of defectors and cooperators among all partners, whether old or new, recognized as old. To correct for chance performance, we present the simple difference between accuracy rate and chance level, averaged across participants.

We found that, on average, defectors were remembered better than cooperators in the defectors-rare condition (h = 0.80), cooperators remembered better than defectors in the cooperators-rare condition (h = 0.80), and both partner types were remembered equally well in the equal-proportion condition (Figure 2.5). This matches the predictions of the rarity hypothesis. Analysing the data at the individual's level showed that this pattern held for most of the participants: 89% of participants in the defectors-rare condition remembered defectors better than they remembered cooperators, 93% in the cooperators-rare condition remembered cooperators better than they remembered defectors, and 84% in the equal-proportion condition remembered both partner types equally well.

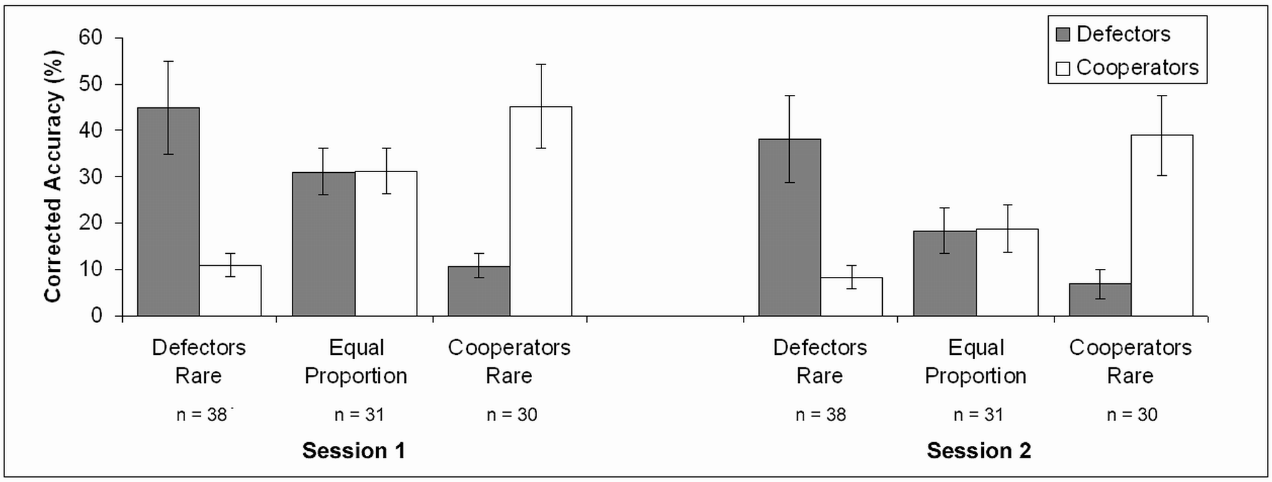

Figure 2.5. Accuracy rates for defector and cooperator partners. For the depicted accuracy rates, we calculated categorization accuracy in conjunction with correct recognition of old partners per participant, subtracted individual chance levels (i.e., the perceived proportion of partner types

among old and new partners recognized as old), and averaged across participants. Error bars represent 95% confidence intervals.

Session 2

Correcting the accuracy rate for the chance level per person in Session 2 resulted, on average, in defectors being remembered better than cooperators when defectors were rare (h = 0.75), cooperators remembered better than defectors when cooperators were rare (h = 0.80), and both partner types remembered equally well when they were equally common (Figure 2.5). This was true for most participants: 92% in the defectors-rare condition remembered defectors better than they remembered cooperators, 93% in the cooperators-rare condition remembered cooperators better than they remembered defectors, and 77% in the equal-proportion condition remembered both partner types equally well. Again in Session 2, the accuracy rate corrected for the chance level supported the predictions of the rarity hypothesis. Across sessions, the accuracy corrected for chance slightly decreased for cooperator and defector partners in all conditions.

Discussion

To explore whether people better remember cheaters or the rare partner type, we varied the proportion of defectors and cooperators (represented by Tit-For-Tat partners) in the interaction group in a between-subjects design and tested whether a cheater-memory or a rarity strategy matched the accuracy rates of partner categorization in conjunction with correct recognition better. Accounting for the perceived proportion of partner types in each condition, in the short (after 10 min) and long run (after 1 week), participants remembered the rare partner type in the interaction group better than they remembered the common one. This pattern of results matches the predictions of the rarity hypothesis.

Our study extends the work on the rarity strategy in cooperation (Barclay, 2008, Bell et al., 2010) by addressing two issues: the role of experience and long-term memory retention.

Does experience influence categorization?

Rather than reading about their partners' behaviour, our participants experienced the partner types in repeated interactions. This way, they could form their own impressions, were personally involved by receiving the payoffs from these interactions, and had the opportunity to establish reciprocal relationships in a more ecologically valid situation. Cooperation with the cooperators increased over the course of 10 repetitions (Figure 2.3), confirming that participants require the repeated interactions to become acquainted with the partner types. This resulted in high recognition and categorization rates (even after a week). The recognition rates we observed are higher than those in previous studies on partner-type memory by 13–65% (Appendix Table B1). Compared to other studies collecting categorization accuracy in conjunction with correct recognition of old partners (Barclay, personal communication; Chiappe et al., 2004, Farrelly & Turnbull, 2009), our conjunction-categorization rates exceed the rates of these studies by 8–68% (Table B1). Some studies reported categorization accuracy of old partners independent of correct recognition (e.g., Barclay, 2008). If we calculate this independent categorization accuracy (Appendix Figure B1), our rates differ by -11–50% (Table B1). Remembering the moves from repeated interactions with partners whose strategies react to one's own behaviour seems to be a difficult task—more difficult than remembering one move per partner as in the previous studies. Nevertheless, repetition pays off by allowing a stronger encoding of partners and an accurate summary of their behaviour. The assignment of types allows individuals to ignore occasional defections of cooperator partners, for example, as long as the cooperator partners, in general, cooperate. This robustness towards variance in the partner's behaviour also applies to memory errors, which makes the assignment of types a good strategy with high memory load. To accurately categorize partners, it seems, repeated interactions are an important component.

How robust are the memory strategies to longer retention intervals?

Barclay (2008) and Bell et al. (2010) found support for the rarity strategy in a memory test minutes after the presentation of the partners. We confirmed this finding and replicated the result in a memory test 1 week after the initial presentation. So, despite this long retention interval, participants performed similarly in the first and second session in correctly recognizing previously seen partners and categorizing defectors and cooperators. This is consistent with the idea that categorizing partners into types is a stable criterion that lasts longer than an immediate repeated encounter. Though accuracy levels decreased slightly across the sessions, this did not seem to interfere with the rarity strategy—approximately the same number of participants showed a preferential memory for the rare partner type in the first and second session. This means that the relation between categorization rates (in conjunction with correct recognition) and chance levels used to correct these rates must be similar. As the chance levels represent participants' perception of the proportion of partner types, one can conclude that not only is participants' memory for partner types robust to longer retention intervals but also their memory for the environment. Compared to other studies collecting categorization accuracy in conjunction with correct recognition of old partners, our conjunction-categorization rates from the second session after 1 week exceed the rates from studies with retention interval of several minutes by 10–47% (Table B1). Some studies reported categorization accuracy of old partners independent of correct recognition (e.g., Barclay, 2008). If we calculate this independent categorization accuracy (Figure B1), our rates from 1 week retention differ from the study with several minutes retention by -8–37% (Table B1). So, our results from the long retention interval of 1 week even mostly exceed those results from studies with several minutes retention, emphasizing the unusual robustness of our categorization results. Moreover, participants benefited from their experience with and improved knowledge of the partner types, indicated by a comparable proportion of cooperation with cooperators in the beginning of the second session as in the end of the first session (Figure 2.3). These findings speak to the importance of memory as one of the prerequisites for establishing long-lasting social interactions via reciprocity and promoting the emergence of cooperation.

Alternative analytical methods

Additionally to addressing the two questions mentioned above, our study, compared to previous ones, employed a different analytical method. Would we still find a rarity effect if we analysed the data with previously used methods (Appendix Document B3)?

Compared to Barclay (2008), our method differs in three aspects—the accuracy rates to test the hypotheses on, the chance level for which to account the accuracy rates, and the way how to account for the chance level. First, whereas Barclay investigated the cheater-memory and rarity strategy in the categorization accuracy independent of correct recognition, we calculated the categorization accuracy in conjunction with correct recognition of old partners, because we consider this a necessary requirement for partner categorization in everyday life. So, as opposed to Barclay, our accuracy rates do not contain categorizations of old partners falsely recognized to be new. Second, that is why, for chance level, we did not take into account the perceived proportion of partner types among old and new partners, like Barclay did, but the perceived proportion among old and new partners recognized as old. Third, Barclay calculated the difference between accuracy rate and chance level relative to the individual chance levels using [(accuracy - chance level) / chance level], but this relative correction biases the difference score in favour of the rare type, increasing the probability of finding a rarity effect. We subtracted the individual chance levels from the accuracy rates to yield a less biased measure. So, our method constitutes categorization accuracy in conjunction with correct recognition of old partners, a chance level of the perceived proportion of partner types among old and new partners recognized as old, and the correction for chance performance by taking the simple difference between accuracy and chance level. Barclay's method constitutes categorization accuracy for old partners independent of correct recognition, the perceived proportion of partner types among old and new partners, and the correction for chance by taking a relative difference. Regardless of the method applied, though, our data always produce a rarity effect (Figure B1).

Bell et al. (2010) analysed their data with the aid of multinomial processing tree (MPT) models. This method distinguishes recognition, categorization, and various guessing biases (Batchelder & Riefer, 1990). Employing a model by Bayen, Murnane, and Erdfelder (1996), Bell et al. (2010) found a rarity effect also in a reanalysis of Barclay's (2008) data. Though we attempted an analysis with our data, we could not apply the MPT method. One guessing assumption MPT models incorporate is the categorization of new partners falsely recognized as old, but participants in our study discriminated old from new partners so accurately that we only had few data points for this category in all conditions and sessions. With scarce data for this guessing assumption, the MPT model could not produce precise estimates for our categorization parameters. Although scarce data distort the analysis with MPT models, this is not a disadvantage of the study. We believe our participants discriminated old from new partners so well, because they were acquainted with them through the repeated interactions. This large amount of experience offers a more realistic situation compared to meeting the partner once, like in a one-shot game. Therefore, the proportion of new partners falsely recognized as old, alone, may not be an appropriate guessing assumption for data with high recognition.

Limitations

The design of our study is limited in some ways that could potentially influence the results. First, rather than cooperating unconditionally, our cooperator partners played Tit-For-Tat and, therefore, were not as easily identifiable as cooperators. Frequently defecting participants did not experience much cooperation by cooperator partners and might not realize that these partners are cooperators, reflected by the large variability of cooperativeness evaluations of cooperator partners (Figure 2.4). Experiencing cooperator partners as cooperative, however, is crucial for categorizing them correctly and can otherwise decrease categorization accuracy. The alternative, implementing unconditionally cooperating partners, could have resulted in greater disadvantages. Participants might defect with these pure cooperators, because there are no costs of exploiting them. In effect, participants would lose the motivation to track partner types which potentially would have decreased categorization accuracy.

Second, whereas letting participants take part in a game increases commitment, a Prisoner's Dilemma game offers only a limited model of cooperative interactions outside the lab. This limits the generalizability of our results. First, according to the definition by Cartwright (2000, p. 86), reciprocal altruism is time-delayed mutualism—donor and recipient of cooperative acts alternate in their roles so that there passes a certain amount of time between the tit and the tat. In the Prisoner's Dilemma we used, however, the exchange of actions happened simultaneously so that a participant was donor and recipient at the same time. Thus, using a sequential Prisoner's Dilemma that enables the alternation of the roles might have modelled the situation we actually wanted to investigate more closely (Frean, 1994, Nowak & Sigmund, 1994). Second, the setup of the game has to be chosen with care: The payoff matrix can influence the behaviour of participants (Furlong & Opfer, 2009), and using computer instead of human partners affects participants as well (Sanfey, Rilling, Aronson, Nystrom & Cohen, 2003).

Conclusion

Our study confirms evidence of a general strategy to remember the rare interaction partners—also in repeated encounters, over a long retention interval, and regardless of the analytical method applied. We reject the cheater-memory hypothesis: In this study, cheaters are not remembered preferentially regardless of the environment. Given that the rarity and cheater-memory hypotheses make the same predictions when defectors in the interaction group are rare, the cheater-memory strategy could be the implementation of the rarity strategy in this kind of environment, though. Contrary to always remembering the same partner type (i.e., cheaters) regardless of the environment, however, the strategy seems to be to remember the type that is rare in the respective environment. Our findings, thereby, support the idea of a cognitive architecture that flexibly responds to the environment instead of specializing in certain interaction groups. By applying the toolbox metaphor of the "fast and frugal heuristics" programme (Gigerenzer, Todd & The ABC Research Group, 1999), our findings suggest that rather than using the same tool (i.e., remember the same partner type) in all possible environments, participants responded in an ecologically rational way by remembering partner types differentially depending on the environment (Todd & Gigerenzer, 2007).

Moreover, our results have implications for the design of new strategies to explain the emergence of cooperation. The traditional reciprocal strategies such as Tit-For-Tat require remembering the partners' single last action and do not distinguish in memory accuracy between defection or cooperation behaviour. Stevens et al. (2011) showed that, when asked to remember the single last action, individuals do not preferentially remember cooperation or defection. Our results, on the other hand, indicate that memory can differentiate between the behaviour in partner types. The combination of these findings leads the way to more realistic strategies that store partner types instead of single actions and distinguish in memory accuracy between defectors and cooperators depending on the environment. Research on indirect reciprocity has already produced strategies acting on the partner's reputation as acquired in the interactions with third parties (Leimar & Hammerstein, 2001, Roberts, 2008). In evolutionary simulations, these strategies outcompeted their opponents and promoted the evolution of cooperation.

In sum, repeatedly meeting interaction partners seems to improve partner-type memory, as we found higher recognition and categorization (dependent and independent of correct recognition) rates compared to previous studies. This strong encoding of partner types could explain the high accuracy rates and the robustness of the memory strategy even after a retention interval of 1 week. Our results suggest that the rarity of defectors and cooperators in the environment influences how well they are remembered. It looks as if people indeed try to minimize costs—not the costs associated with exploitation, as suggested by the cheater-memory hypothesis, but the costs associated with memory errors. Of two people with whom you interacted, the cheater might be the more important partner type to remember, but in an environment where cheaters represent the majority, the costs for remembering all of them overrule the costs of exploitation.

Chapter 3
Not impressive—On the role of impression-based strategies in the emergence of cooperation

Preliminary note

Chapter 3 is the result of a collaboration with Sebastian Scholz. We would like to thank Jeffrey R. Stevens for his support and guidance in the beginning of the project and helpful comments on an earlier draft, the ABC research group and especially Nadine Fleischhut and Henrik Olsson for thought-provoking discussions, and Henrik Olsson and Paul Leiber for helpful comments on an earlier draft of the manuscript.

Abstract

In this project, we are looking for strategies to explain the emergence of cooperation that are robust to noise (i.e., memory or perception/decision errors). Tit-For-Tat, the most prominent example of 1-step memory strategies, is not robust to noise, because even little noise decreases its success. Noise is quite common in everyday life, though. The strategies' concept of a 1-step memory is a too-narrow basis to successfully cope with noise. That is why authors proposed strategies that rely on a larger part of the common history between player and partner. This is a more stable criterion on which to base the decision, because, for example, occasional defections from a generally "good" partner do not lead to immediate retaliation. Yet an alternative are non-contingent strategies that do not act upon their partners' behaviour and are thus not susceptible to noise. We test the three strategy groups competitively regarding their robustness to noise, allowing for a vast array of strategies and stochasticity.

We, first, hypothesize that impression-based strategies are more robust to noise than 1-step memory strategies in maintaining cooperation. To avoid the bias of a strategy set chosen by the experimenter, we let strategies evolve in a genetic algorithm simulation. Complemented by mutation and crossing over, successful strategies survive and spread, whereas unsuccessful ones vanish. The simulation supports the hypothesis. Most robust to noise were non-contingent strategies, though.

Second, we hypothesize that impression-based strategies better predict human behaviour than 1-step memory strategies. We cross-validated the strategies on participants' behaviour and confirmed the hypothesis. Again, non-contingent strategies were most successful in predicting participants' behaviour.

The results suggest that non-contingent strategies are more successful in the simulation and better predict participants' behaviour than contingent strategies. This implies that humans pursue even simpler strategies than impression-based and 1-step memory strategies.

Introduction

Which strategies do people pursue that help emerge and maintain cooperation? One of the first attempts to answer this question was Axelrod's (1980a) famous tournament in which he pitted different strategies against each other and compared their success in playing the Iterated Prisoner's Dilemma game. There, a player and his partner can either cooperate or defect and receive payoff depending on both choices. Whereas defection yields more payoff for each individual, mutual cooperation is the best result for both opponents. To qualify as a Prisoner's Dilemma, the payoffs have to adhere to the size order: one-sided defection > mutual cooperation > mutual defection > one-sided cooperation. Additionally, alternating defection and cooperation must not exceed the averaged payoff from mutual cooperation.

In Axelrod's (1980a) tournament, Tit-For-Tat (TFT) won, a strategy that begins by cooperating and then reciprocates the partner's previous move. TFT also won a second tournament, an evolutionary simulation (Axelrod, 1980b), and a genetic algorithm simulation by Axelrod (1987). Soon, this strategy became well-known, not at least because of its simple and comprehensible eye-for-an-eye style. It was difficult, though, finding evidence for the implementation of TFT in non-human animals (Milinski, 1987 and Dugatkin, 1991, but Lazarus & Metcalfe, 1990, Masters & Waite, 1990, Reboreda & Kacelnik, 1990, and Stephens, Anderson & Benson, 1997; Lombardo, 1985, but König, 1988 and Lombardo, 1990) or humans. Oskamp's (1971) review of Iterated Prisoner's Dilemma results reports three studies showing that, when participants played the game against each other, the cooperation rate was lower compared to that of TFT. Wilson (1971) and Opp (1988) confirmed this finding. As reported by Erev and Roth (1998), even Anatol Rapoport, who proposed TFT in Axelrod's (1980a, 1980b) tournaments, did not find evidence that his participants used TFT. Also with Wedekind and Milinski (1996, Milinski & Wedekind, 1998) the majority of participants pursued a more non-cooperative strategy.

One reason for this discrepancy between theory and practice was that authors had neglected TFT's susceptibility to noise (see Axelrod, 1984, on the effects of noise on TFT's success). We will use the term noise to refer to memory errors (i.e., a player cannot remember what his partner did; as in Milinski & Wedekind, 1998, Stevens et al., 2011) and perception or decision errors (i.e., a player misinterprets his partner's actions or, for some reason, does not respond in the corresponding fashion; as in Boyd, 1989, Nowak & Sigmund, 1993, Wu & Axelrod, 1995). A single error between two TFT players results in alternating defections, largely decreasing their success. Besides studies with stable noise levels (Donninger, 1986, Bendor, Kramer & Stout, 1991, Lindgren, 1991), analytical derivations (Molander, 1985, Bendor, 1987, 1993, Müller, 1987) and studies with varying noise levels (Wu & Axelrod, 1995, Lomborg, 1996, Stevens et al., 2011) proved that TFT is not robust to noise. Because people are prone to memory, perception, or decision errors, TFT cannot be deemed a realistic candidate to explain human behaviour.

Proposals for noise-robust strategies

Authors made different proposals which strategies work best in a noisy environment. In Nowak and Sigmund's (1993) simulations, Win-Stay, Lose-Shift won (to not confuse it with another strategy, we will refer to it as Win-Stay, Lose-Change, WSLC, as did Messick & Liebrand, 1995). WSLC reciprocates cooperation only if both, player and partner, cooperated on the previous move (and defects if either of them defected). On the other hand, it cooperates after mutual defection to offer a way back to cooperation. In other simulations, WSLC was not as successful (Wu & Axelrod, 1995, Stevens et al., 2011). Although having never won analytical derivations or simulations, Grim Trigger (Grim) is deemed a good strategy to establish cooperation in the presence of non-cooperative strategies (Müller, 1987, Nowak & Sigmund, 1993). With Grim, a player cooperates as long as the partner cooperates and defects indiscriminately after the partner's defection. Another strategy proposed for noisy environments is TFT with occasional unconditional cooperation (called "generous"). Generous TFT proved to be superior to TFT what concerns robustness to noise (Molander, 1985, Bendor et al., 1991, Nowak & Sigmund, 1992, Wu & Axelrod, 1995). That seems to be true only up to a certain degree of noise, though. In Stevens et al.'s (2011) simulations with a broader range of noise, all variants of TFT and also other promising strategies without stochasticity (WSLC, Grim) vanished, and cooperation went extinct.

The strategies, we have mentioned so far, have in common a 1-step memory. We believe this is a too-narrow basis to successfully cope with noise. Stevens et al. (2011) showed that participants made mistakes when asked to recall their partners' single last move. At the error rate they found, cooperation in an evolutionary simulation vanished. For more robustness to noise, authors proposed strategies that rely on a larger part of the common history between player and partner. Bendor (1987), for example, explained the advantage of long-term memory strategies especially in a noisy environment by pointing out the resistance to outliers from behaviour shown so far: "In a sense, that history creates a fund of goodwill or trust that is not obliterated by one bad encounter. Conversely, of course, one good episode will not overwhelm a long memory of bad times”. Being better equipped against noise, we hypothesize that strategies relying on more information from the history with the partner are not only more robust to noise than 1-step memory strategies but also model human behaviour more closely.

Another way to be robust to noise is not to rely on noise-susceptible information. Non-contingent strategies do not base their decision on their partners’ behaviour but determine whether to cooperate on each move with a certain probability (e.g., AllD defects on each move). Although this is an intriguing method against noise, it contradicts the idea of reciprocity as one of the mechanisms proposed for the emergence of cooperation in humans (Trivers. 1971).

When Axelrod (1984) wanted to find the best strategy for the Iterated Prisoner’s Dilemma, he realized that there is no single best strategy independent of the environment (i.e., the competing strategies). This explains why the strategies proposed as noise-robust alternatives to TFT were successful in some studies and not successful in others. Only in an environment with as many competitors as possible are noise-robustness results reliable. One-step memory strategies and non-contingent strategies have been tested against each other regarding their robustness to noise (e.g., Nowak & Sigmund, 1992). Strategies relying on more information from the history with the partner have been tested separately on their robustness to noise (e.g., Nowak & Sigmund, 1998). There have been studies testing the three strategy groups competitively under noise but only with a limited set of strategies (e.g., Anh et al., 2011) or not allowing for stochasticity in the strategies (Lindgren, 1991, Miller, 1996). Here, we test the three strategy groups competitively under noise, allowing for a vast array of strategies and stochasticity. To avoid the bias of a strategy set chosen by the experimenter, we used a genetic algorithm as the method to competitively test all three strategy groups.

Testing strategies’ robustness to noise

One way of building an environment as diverse as possible without having the experimenter choose the set of strategies is with a genetic algorithm (Holland, 1975). A genetic algorithm is a search method for a multidimensional space. The space consists of a string of features representing, in this case, various strategies. Applying crossover and mutation allows for strategies to adapt; letting successful strategies grow in their number of members and less successful ones cease results in the evolution of the most successful strategy. Since Axelrod (1987), some studies using genetic algorithms in the Iterated Prisoner’s Dilemma did not deviate from the fixed memory size of the original (Marks & Schnabl, 1999, Golbeck, 2002, Dyer, 2004). Other studies let strategies with different memory length evolve but did not include noise (Crowley, Provencher, Sloane, Dugatkin, Spohn, Rogers & Alfieri, 1996, Scali, 2006, Brunauer, Löcker, Mayer, Mitterlechner & Payer, 2007). Lindgren (1991) and Miller (1996) represented strategies by a genetic algorithm/finite automata with variable memory size and let them evolve in a simulation with noise. Their design did not allow stochastic strategies to evolve, though. We designed a genetic algorithm with variable memory size able to represent 1-step memory strategies, strategies relying on more information from the partner's past, and non-contingent strategies. Moreover, it allows for stochasticity. We ran this genetic algorithm in an evolutionary simulation with increasing noise.

Impression-based strategies

For strategies relying on more information from the partner's past, we designed impression-based strategies. These strategies determine an impression of the partner and cooperate as long as the impression is "good" and defect if the impression is "bad". A player determines his partner's impression by updating an impression index. This impression index is based on the outcome of the past interaction (i.e., the own move and the partner's move) to which the player assigns a certain weight. Refining the above statement, the player cooperates if the impression index is positive and defects if it is negative.

There have been ideas for how the updating of such an index may proceed. In the areas of person cognition, and attitude theory and attribution, Gollob, Rossman, and Abelson (1973), for example, reported an additive model of social inference. This model holds that one can infer a person's attitude towards one issue by summing his attitudes towards a related issue. Anderson and Birnbaum (1976), however, refuted the additive model with its assumption of independent value and number of instances in favour of an averaging model. Although averaging here rather refers to a weighted serial integration process.

In studies concerning the emergence of cooperation, the idea of adding 1 if the partner cooperates and subtracting 1 if he defects was used in the image scoring literature (Nowak & Sigmund, 1998, Leimar & Hammerstein, 2001, Engelmann & Fischbacher, 2009). There, the focal person has to remember information on how the partner behaved in interactions with third parties, which is an additional source for noise. Intention recognizers in Anh et al.' (2011) model use a Bayesian network to assess their partners' intention, but the conditional probability of the past observations given the intention is based on a simple subtraction of defective from cooperative moves which is then normalized. Whether image scoring or intention recognition, adding or subtracting 1 neglects the dimensions different acts of defection and cooperation can take. Wakano and Yamamura (2001) reported a learning strategy that acts on an internal state and updates this state based on the interactions: It reinforces cooperation or defection when the payoff from the interaction exceeds an aspiration level and negatively reinforces the behaviour when the payoff undercuts the level.

Among the strategies Bendor (1987) analysed in an Iterated Prisoner's Dilemma with noise was a weighted average strategy (again, rather a weighted serial integration) with variable memory. Erev and Roth (2002) and Hruschka and Henrich (2006) reported strategies which used an index, either to reinforce a certain strategy from a set of strategies or to decide about the preference of partners for future interactions. Both models updated the index by averaging with a weighted payoff from the current move. Alonso-Sanz and Martin (2006) showed that in a simulation with a spatial setup a strategy averaging over three past steps boosted cooperation.

Based on previous research, we implemented an "Adding" and an "Averaging" impression-based family. Adding sums weights assigned to the moves to determine an impression index. Averaging builds the average of a certain window of past weights. Additionally, we implemented a "contradictory Adding" (ConAdding) and a "contradictory Averaging" (ConAveraging) strategy family. In contrast to the "regular" forms which take all of the partners' moves into account to determine an impression index, the contradictory variants only consider the moves that contradict the current impression. In line with reciprocity, these strategies do not update a positive impression index, for example, as long as the partner continues to cooperate but only take defective moves into account, in this case, to reciprocate defection immediately. Thus, contradictory strategies are more provocable and less forgiving than regular strategies.

With Adding (and ConAdding), a person arrives at an impression by storing a single updated impression index, the sum of all moves' weights. Directly after the interaction, the player adds to this sum the present move's weight so that he has to keep in mind just the single updated impression index. Anderson (1996) described the integration of adjectives into a judgement of a person and pinpoints what we mean by impression formation via Adding—if one replaces "adjective" by "move of the partner":

As each successive adjective is given, the subject evaluates it and integrates this

> valuation into the accumulative judgement memory of the person. Once its meaning has been extracted and integrated, the adjective itself is no longer necessary. It may be stored separately or forgotten. The main functional memory is the integrated memory about the person. (p. 366)

With Averaging (and ConAveraging), a person stores the moves' weights in a certain memory window and acts on the average of them. If memory for the previous move (cooperate [C] or defect [D]) is error prone, as in Stevens et al.'s (2011) experiment, one might argue it will be even more error-prone for moves before the previous one. Still, an average of several noisy moves might be a better basis for an appropriate reaction than a single last noisy move. Impression-based strategies act on a negative/positive scale, and even if a player erroneously remembered or perceived some negative weights along with the positive weights from interactions with a cooperator, chances are the average will still be positive so that the player cooperates. On the other hand, a strategy acting on the partner's previous move defects with a cooperator if the player erroneously remembered or perceived a cooperator to have defected. Moreover, people might be bad in remembering the exact outcomes of the individual interactions—and therefore 1-step memory strategies ask them to do something they do not do in reality—because that is not how memory works in everyday life. Instead, they might be able to process the outcomes and arrive at an average/impression that represents the partner's type quite accurately. In perception research, participants estimated the mean size of symbols more accurately than the size of the individual symbols (Ariely, 2001, Chong & Treisman, 2005). Studies in the judgement/impression formation literature showed that the processing of adjectives or behaviour is independent of memory (Anderson & Hubert, 1963, Dreben, Fiske & Hastie, 1979). Whereas memory for individual adjectives changed after a distraction task, the judgement remained the same (Riskey, 1979).

So, the impression index is a more stable criterion compared to just the single last action, because noise does not necessarily result in inappropriate reactions: Occasional defections from a generally "good" partner do not lead to immediate retaliation and occasional cooperative moves from a generally "bad" partner do not lead to immediate forgiving.

In sum, we hypothesize that

1. Impression-based strategies are more robust to noise than 1-step memory strategies in maintaining cooperation and surviving in an evolutionary simulation.

2. Impression-based strategies better predict human behaviour than 1-step memory strategies.

We tested three strategy groups, 1-step memory (e.g., TFT, WSLC, Grim), impression-based (Adding, Averaging, ConAdding, ConAveraging), and non-contingent strategies (e.g., AllD), competitively under noise. To assess whether impression-based strategies are more robust to noise than 1-step memory strategies, we ran a genetic algorithm simulation with varying noise. To evaluate whether impression-based strategies better predict human behaviour than 1-step memory strategies, we cross-validated strategies from all three groups on behavioural data from a previous project (Volstorf et al., 2011).

Methods

Functionality of impression-based strategies

The main idea of impression-based strategies is to act on an impression of the partner. The impression is represented by an impression index that refers to a specific value along a negative-positive scale. Positive impression indices represent a "good" impression, negative impression indices represent a "bad" impression. The initial impression of a partner is indifferent (0). After a player has interacted with his partner, he updates the impression index of the partner in a first step and, second, acts on it in their next interaction. The decision rule is: "cooperate if the impression index is positive, defect if the impression index is negative". A player, for example, that has updated the partner's impression index to be 2 after their first interaction has a "good" impression of the partner and will cooperate in their next interaction. A player that has updated the impression index to be -4 has a "bad" impression and will defect in their next interaction. We call the values to update an impression index "weights". One can think of them as the player's evaluation of an interaction's outcome.

Besides the regular forms—Adding which sums the weights and Averaging which averages the weights of a certain window of past interactions—we distinguish a contradictory Adding and a contradictory Averaging family. Contradictory forms just consider the weights of moves that contradict the current impression. Imagine, the weight for mutual cooperation is 2, and the weight for unilateral defection by the partner is -3. With Adding, a player who cooperated with his partner for five times has an impression index of (2 + 2 + 2 + 2 + 2 =) 10 (Averaging with window size 3: (2 + 2 + 2) / 3 = 2). If the partner defected in the sixth interaction, the impression index will be (10 + [-3] =) 7 (Averaging: (2 + 2 + [-3]) / 3 = 0.33), and the player will continue to cooperate. With ConAdding, a player who cooperated with his partner for five times has an impression index of (2 =) 2 (ConAveraging with window size 3: 2 / 1 = 2). If the partner defected in the sixth interaction, the impression index will be (2 + [-3] =) -1 (ConAveraging: (2 + [-3]) / 2 = -0.5), and the player will defect. If the defection by the partner was intended, contradictory strategies will not let him get away with it, whereas Adding and Averaging risk being exploited. If the defection was due to noise, Adding and Averaging will overlook it and move on, whereas contradictory strategies risk decreasing payoff through needless defections.

The chromosome

We ran a simulation in which agents meet to play the Iterated Prisoner's Dilemma game (Table 3.1).

Table 3.1

Payoff for the Prisoner's Dilemma game.

Player's Choice	Partner's Choice	
	Cooperate	Defect
Cooperate	3 ; 3	0 ; 5
Defect	5 ; 0	1 ; 1

Note. Payoff on the left in each cell is paid to the player, on the right to the partner.

We did not distribute certain strategies among the agents, however, but equipped each agent with features that, in combination, result in a strategy and of which parameters are determined randomly at the beginning of the simulation. This kind of simulation is called a genetic algorithm and the features, following the biological metaphor, are called genes. There were eight genes in a chromosome that determined an agent's strategy (Figure 3.1).

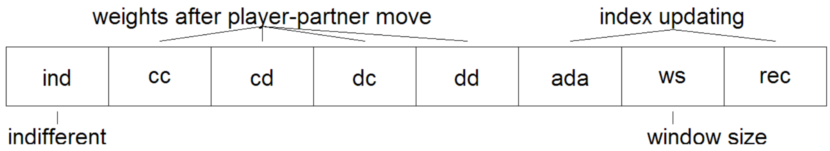

Figure 3.1. Genes in the chromosome and their meaning. Gene "ind" gives the probability to

cooperate whenever the player is indifferent, that is, whenever the impression index is 0. Genes "cc", "cd", "dc", and "dd" refer to the weight put on what player and partner did on the previous move. Genes "ada" (0 = adding, 1 = averaging) and "rec" (0 = regular, 1 = contradictory) determine how a strategy updates an impression index. Gene "ws" gives the window size of past interactions for strategies with ada = 1.

The first gene ("ind") codes the probability of cooperating if the impression is indifferent —that is, on the first move between a player and his partner and every time the impression index becomes 0 again. The next four genes represent the weights an agent applies to the situations of mutual cooperation (CC) in the previous interaction (gene "cc"), "player cooperated and partner defected" (CD; gene "cd"), "player defected and partner cooperated" (DC; gene "dc"), and mutual defection (DD; gene "dd"). These weights range from -5 to +5. Gene "ada" determines whether the strategy adds (0) or averages (1); gene "rec" determines whether the strategy is of regular form (0) or a contradictory variant (1). So, genes ada and rec represent the membership of one of the four strategy families of impression-based strategies: Adding (ada = 0, rec = 0), Averaging (ada = 1, rec = 0), ConAdding (ada = 0, rec = 1), and ConAveraging (ada = 1, rec = 1). Gene "ws" gives the window size an agent looks back in the history of the partner's moves (i.e., weights) with him and ranges from 0 to 9 (for 10 interactions). Only Averaging/ConAveraging strategies make use of the window size. In the remaining section, we will describe basic principles of defining strategies with the chromosome. For definitions of strategies, please consult Appendix Table C1.

The chromosome can also represent 1-step memory strategies like TFT, WSLC, or Grim, and non-contingent strategies like AllC (always cooperate), AllD, or Random (determine randomly whether to cooperate or defect). To model these strategies, the chromosome entails that also 1-step memory and non-contingent strategies are defined by impression-based features (genes ada, ws, rec) and build an impression index. Crucial, however, is not the genotype, the genetic disposition, but the phenotype, the shown behaviour. If all weight genes are 0, for example ("0–100, 0, 0, 0, 0, *, *, *", where * means irrelevant), the impression index of the partner stays indifferent after each interaction. In this case, the strategy always goes by gene ind, and all other genes are irrelevant. The same is true if ada = 1 (Averaging family) and the window size is 0 (here, the weight genes can be ≠ 0: "0–100, *, *, *, *, 1, 0, *"). This strategy does not build an impression index. Instead, gene ind determines with which probability the agent cooperates on every move. This enables categorizing non-contingent strategies. Assigning the probability space of ind from 0 to 100 to strategies, we distinguished AllD (ind = 0), Random (ind = 50), AllC (ind = 100), and four more strategies (e.g., C_1–25 with ind = 1–25 cooperates in 1 to 25% of the cases).

Non-contingent strategies are immune to noise—they do not take their partners' behaviour as a basis for their action, so they cannot implement their move incorrectly based on a misremembered or misperceived partner's move. The "0–100, *, *, *, *, 1, 0, *" chromosome is the only noise-robust category for non-contingent strategies, because literally no impression index is built that could be misremembered, misperceived, or implemented incorrectly. All gene constellations, including those that build an impression index, have to be assigned to strategies, though, and non-contingent strategies that build an impression index are automatically noise-prone. That is why we had to compromise and defined noise-prone non-contingent strategies (probCoop, probExtremeAlternate, probMoody, probDef; see Table C1). Again, building an impression index with non-contingent strategies is an artefact of the chromosome; these strategies phenotypically still qualify as non-contingent strategies. So, with the onset of noise, we introduced two changes: a) reduce the categorizations of each non-contingent strategy to the "0–100, *, *, *, *, 1, 0, *" chromosome so that it is independent of noise and still does what its name promises (AllC, AllD, Random, C_-strategies), b) if a) is not possible, subsume the categorizations under probabilistic non-contingent versions that are noise-prone. The latter was the case for nine non-contingent strategies, four C_1st-strategies (which cooperate on the first move with ind% and cooperate from then on), four D_1st-strategies (which cooperate on the first move with ind% and defect from then on), and ExtremeAlternate (which cooperates or defects on the first move and does the opposite of its first move from then on). These strategies, together with the noise-prone categorizations of the remaining non-contingent strategies, merge into probabilistic non-contingent strategies (all C_1st-categorizations and one AllC-categorization merge into probCoop, ExtremeAlternate merges into probExtremeAlternate, some AllC-, AllD-, Random-, C_-categorizations merge into probMoody, all D_1st-categorizations and one AllD-categorization merge into probDef).

We defined 1-step memory strategies (like TFT, WSLC, Grim) by ada = 1 with ws = 1, because these build an impression index by only considering the weight from the last encounter. Moreover, 1-step memory strategies can be described by rec = 1 (with ws = 2). Here, we made use of the "contradictory" feature: These strategies do not update the impression index if the partner continues to do what he has been doing. Thus, the impression index stays close to the threshold between the impressions and allows an immediate reaction to a contradictory move.

For a strategy's index to, in fact, stay close to the threshold between impressions, we often applied additional rules, though. With the ada = 0, rec = 1 version of TFT, for example, the absolute value of gene cd has to be greater than gene cc so that a player, in fact, defects after CD. For the player to cooperate after DC, we set |cd| = dc.

We defined impression-based strategies by means of the various constellations of weight gene parameters (Table 3.2). The genotypic contrast to 1-step memory strategies is that the impression index is built from more than the last encounter (with Averaging and ConAveraging from more than the last two encounters). Besides the 20 non-contingent strategies, this resulted in 12 subforms for each of the four impression-based strategy families (Adding, ConAdding, Averaging, ConAveraging) and three 1-step memory strategies (TFT, WSLC, Grim). To even out the competition, we created two 1-step memory strategies out of every impression-based subform: one strict form with either ind = 0 or ind = 100 and additional rules for the weight genes to assure the index staying close to the threshold (as with TFT, WSLC, Grim), and a probabilistic form with unrestricted gene ind and rules allowing to cooperate after cooperation or defection of the partner with a certain probability. This resulted in 24 1-step memory strategies. The impression-based subform we named with an "i" (for "impression-based") at the beginning (e.g., iTFT) to differentiate it from the strict form.

Table 3.2

Constellations of weight gene parameters and the respective strategies.

Weight gene parameters	Strategies
cc > 0, cd > 0, dc > 0, dd > 0	probCoop‡
cc < 0, cd > 0, dc > 0, dd > 0	Apologizer, probApologizer, iApologizer†
cc > 0, cd < 0, dc > 0, dd > 0	Cooperator, probCooperator, iCooperator†
cc > 0, cd > 0, dc < 0, dd > 0	Temptation, probTemptation, iTemptation†
cc > 0, cd > 0, dc > 0, dd < 0	Shy, probShy, iShy†
cc < 0, cd < 0, dc > 0, dd > 0	ExtremeAlternate‡, probExtremeAlternate‡
cc < 0, cd > 0, dc < 0, dd > 0	AntiTFT, probAntiTFT, iAntiTFT†
cc < 0, cd > 0, dc > 0, dd < 0	WCLS, probWCLS, iUnilateral†
cc > 0, cd < 0, dc < 0, dd > 0	WSLC, probWSLC, iMutual†
cc > 0, cd < 0, dc > 0, dd < 0	TFT, probTFT, iTFT†
cc > 0, cd > 0, dc < 0, dd < 0	probMoody‡
cc < 0, cd < 0, dc < 0, dd > 0	Lurer, probLurer, iLurer†
cc < 0, cd < 0, dc > 0, dd < 0	GreedyTFT, probGreedyTFT, iGreedyTFT†

cc < 0, cd > 0, dc < 0, dd < 0	Sucker, probSucker, iSucker†
cc > 0, cd < 0, dc < 0, dd < 0	Grim, probGrim, iGrim†
cc < 0, cd < 0, dc < 0, dd < 0	probDef‡

Note. For simplicity, we only display gene parameters ≠ 0. For the cases in which gene parameters can be = 0, please consult Table C1. WCLS = Win-Change, Lose-Stay. All strategies beginning with "i" are impression-based strategies, strategies with ‡ mark non-contingent strategies, the remaining ones are 1-step memory strategies. † Subform with strategies of each of the four strategy families (Adding, ConAdding, Averaging, ConAveraging), e.g., iApologizer_Adding.

The simulation

At the beginning of each run, the programme determined the parameters of the genes randomly. The population consisted of 20 agents, each with his own set of gene parameters. Running the simulation with 40 agents did not change the results but took considerably longer. In the first block of interactions, each agent played the Iterated Prisoner's Dilemma game with everyone else. This procedure was repeated for 10 blocks, with a random order each time. This constituted a generation. Repeating the block for 20 times did not change the cooperation rates.

We implemented noise as the standard deviation of a normal distribution around the correct impression index (with ada = 0) or weight (with ada = 1). That is, with a certain probability the agent did not act on (i.e., remember, perceive, or implement) the correct impression index/weight but neighbouring indices/weights within standard deviations from 0.1 to 5.0 in units of 0.1 (Figure 3.2). The normal distribution equation $f(x) = 1 / (\sigma \sqrt{(2\pi)}) \exp(-1/2 ((x - \mu) / \sigma)^2)$ gives the density curves for the different standard deviations. We calculated the probability for the corresponding x-axis values relative to the sum of all density parameters per curve. Standard deviation (*SD*) 0.4 marked the onset of noise, because the probability to act on the correct impression index/weight is < 1 and the probability to act on impression indices/weights ± 1 from the correct index/weight is > 0. As the standard deviation of the normal distribution around the correct impression index/weight increases, the probability to act on neighbouring indices/weights as well as the number of possible indices/weights increases. At *SD* = 5, the probabilities to act on the correct or a neighbouring impression index/weight only differ minimally.

At the end of a generation, agents reproduced in relation to their success to build the population for the next generation. So, successful strategies potentially grew in their number of members and less successful ones ceased. The programme determined a strategy's success by, first, cumulating each agent's payoff. Then, the programme summed the payoff of all agents and assigned each agent to his share of the sum using the roulette-wheel technique (e.g., if the first agent gained 1 point, the second agent gained 3, then the first agent is assigned to the number 1, the second is assigned to the numbers 2 to 4...; Mitchell, 1998). Third, the programme chose two "parent" agents by drawing random numbers between 0 and the overall sum and picking the assigned agents. With a probability of 70% (corresponds to a crossover rate of 0.7), each gene marked a crossover spot. That is, the two agents switched all genes in their chromosomes following the spot. The programme sequentially determined crossover spots for the first and second agent. Afterwards, with a mutation probability of 0.1% (mutation rate of 0.001), each gene from both agents increased or decreased its parameter by one unit within the corresponding range (e.g., 0–100 in gene ind, 0/1 in gene ada). The two resulting "offspring" agents were placed in the next generation. The programme repeated this survival mechanism "with replacement" until the population was complete. Neither a crossover rate of 0.4 nor a mutation rate of 0.005 changed the cooperation rates. One run of the simulation stopped when each agent had the same parameters for all genes, that is, when all agents in the population pursued the same strategy. This happened after an average of $M = 78.2 \pm 0.1$ generations (± 95% confidence interval). We repeated the simulation for 10,000 runs.

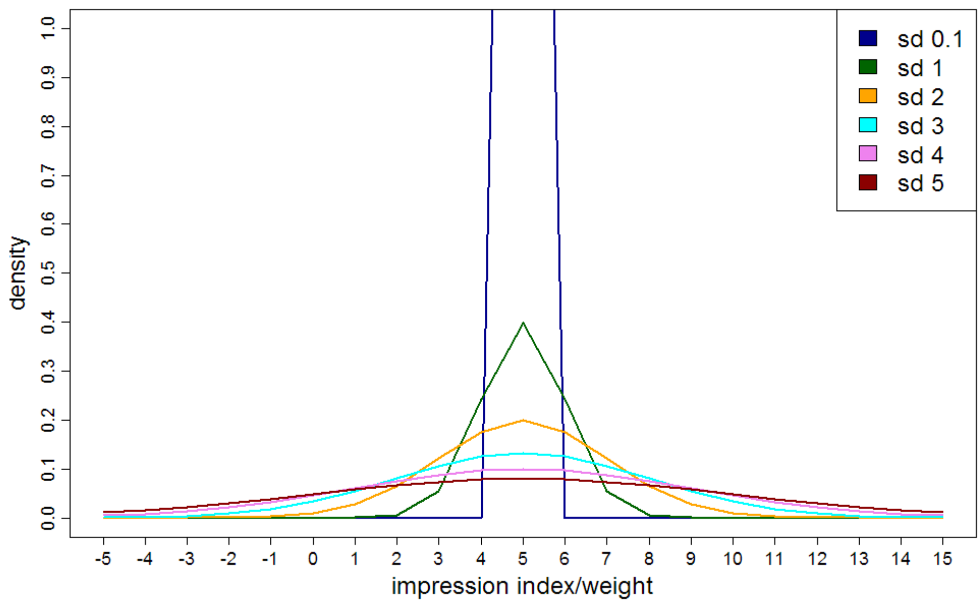

Figure 3.2. Example standard deviations of the normal distribution around the correct impression index (with ada = 0) or weight (with ada = 1), here: 5, used to represent noise in processing the impression index/weight. The y-axis gives the density from which we calculate the probability to act on the impression index/weight (see text).

The cross-validation

After the simulation, we cross-validated a set of strategies on participants' data we had collected for a previous project (Volstorf et al., 2011). There, 126 participants played the Iterated Prisoner's Dilemma game with 20 computer partners in a blocked interaction pattern which was repeated for 10 times. Computer partners either played AllD or TFT. We assigned participants to either of three interaction groups which differed in the proportion of computer partners' types. In the "defectors-rare" condition, 20% of interaction partners defected, 80% cooperated. In the "equal-proportion" condition, 50% defected, 50% cooperated, and in the "cooperators-rare" condition, 80% of partners defected and 20% cooperated. Each type comprised half male and half female partners.

As shown in Table C1, each strategy can be represented by multiple gene constellations. For the cross-validation, we fed in all gene constellations per strategy that won at least one run of the simulation with *SD* = 0.1 (without noise) and *SD* = 5.0 (including noise). When gene constellations were not available, because the strategies had not won in the simulation, we constructed a small parameter set. The latter we did only for non-contingent and 1-step memory strategies, though, because they can be described (and thus cross-validated) independently of the chromosome. At the cross-validation including noise, the programme additionally entailed noise in the form of a normal distribution with *SD* = 5.0 with each cross-validated move.

To evaluate the cross-validation, we used a predictive accuracy measure similar to the accumulative prediction error (Wagenmakers, Grünwald & Steyvers, 2006). Because the data are equivalent to time-series data, one does not take n-1 observations into account to predict the n^{th} observation (i.e., the size of the data set for fitting is constant; leave-one-out cross-validation). Instead, the data set to base the prediction on (i.e., fit the model) grows continuously as one proceeds. For each observation, we made a prediction based on the previous observations and corresponding to each strategy. We assigned a "1" for correct predictions and a "0" for incorrect ones. Instead of summing the errors (0s) and choosing the model (i.e., strategy) with the smallest average error, we summed the correct predictions (1s) and chose the model with the best average prediction. Since most of the strategies decide what to do on the first move probabilistically, we repeated the cross-validation for five times.

Software

We wrote the code for running the simulation in Free Pascal (version 2.4.0). For categorizing the chromosomes into strategies, creating the figures, and cross-validating the strategies on participants' data, we used R (2011, version 2.13.2), and the packages Hmisc (error bars Figure 3.3; Harrell, 2010) and pwr (to calculate Cohen's *h*; Champely, 2009). All programmes are available upon request.

Design and data analysis

To test Hypothesis 1 ("Impression-based strategies are more robust to noise than 1-step memory strategies in maintaining cooperation and surviving in an evolutionary simulation."), we ran an evolutionary simulation using a genetic algorithm. As the independent variable, we varied the noise level, that is, the standard deviation of a normal distribution around the correct impression index/weight (with the effective onset of noise at $SD = 0.4$). As the dependent variables, we assessed the proportions of strategies winning the 10,000 runs of the simulation and the cooperation rate in the population of strategies. Each incident of cooperation (one-sided or mutual) in the generation after a run stopped, divided by all moves, contributed to the cooperation rate. For descriptive statistics, we report mean (*M*), standard deviation (*SD*), range, median (*Mdn*), and mode (*Mo*). For comparisons between results, we give the mean with 95% confidence interval (CI; e.g., Cumming et al., 2007) and either Cohen's (1977) *d* (with means) or Cohen's *h* effect size (with proportions; Cohen's conventions for *d* and *h*: small effect size: 0.20, medium effect size: 0.50, large effect size: 0.80).

To test Hypothesis 2 ("Impression-based strategies better predict human behaviour than 1-step memory strategies."), we cross-validated a set of strategies on participants' behaviour from an earlier experiment (Volstorf et al., 2011). As independent variable, we cross-validated the behaviour without and including noise by feeding in parameters from the simulation with $SD = 0.1$ or $SD = 5.0$. The predictive accuracy of cross-validation, the dependent variable, we determined by dividing the number of correct predictions by the number of cases in which the strategy made a prediction over all of 20 partners, five cross-validation repetitions per participant, and over all gene constellations. By chance (and with the Random strategy), this accuracy is at 0.5. We report three measures of cross-validation performance: First, the frequency with which a strategy best-predicted participants' behaviour (i.e., had the highest predictive accuracy) among the set of strategies, second, the value of predictive accuracy, and third, a combined measure of both.

Results

The simulation

Figure 3.3 shows the results of 10,000 runs of the simulation for each standard deviation representing the level of noise.

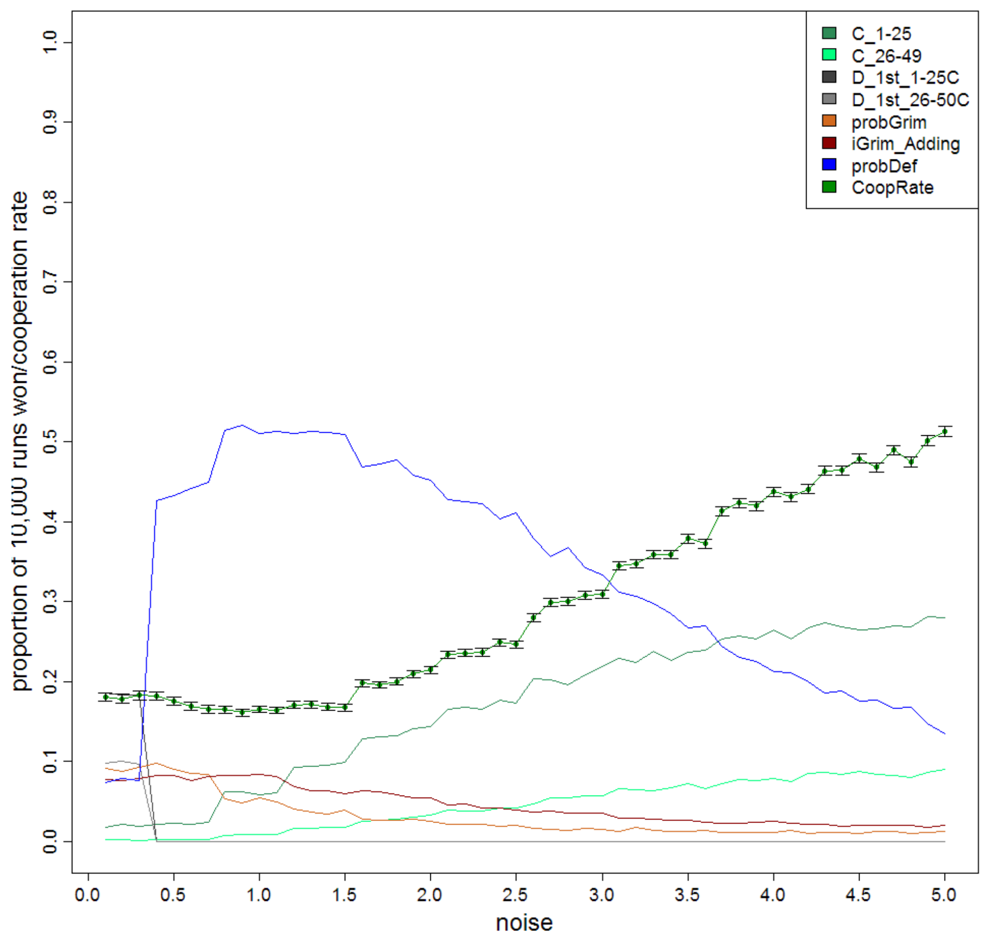

Figure 3.3. Proportion of strategies winning among 10,000 runs over an increasing level of noise (represented by the standard deviation of a normal distribution around the correct impression index/weight). For better clarity, we only display the best seven strategies. The green

line (with error bars) shows the cooperation rate in the generation after a run stopped averaged over 10,000 runs per noise level. Error bars represent 95% confidence intervals.

Although they won a few times (Appendix Figure C2), none of the 1-step memory strategies ever dominated the population of winner strategies over the different levels of noise. Among the seven strategies with the highest proportions of runs won, there was only one 1-step memory strategy, probGrim. ProbGrim ranked third behind two non-contingent strategies (D_1st_1–25C and D_1st_26–50C) at the simulation without noise. The situation for impression-based strategies was similar (Appendix Figure C3). Among the seven best strategies, there was just one impression-based strategy, iGrim_Adding. At the simulation without noise, iGrim_Adding ranked behind probGrim, but they switched ranks shortly after the onset of noise until SD = 5. Aggregating proportions per strategy group and over noise levels, impression-based strategies won more proportions of runs than 1-step memory strategies without and including noise (without noise [SD = 0.1–0.3]: $M_{\text{impression-based}}$ = 0.35 ± 0.00 CI, $M_{\text{1-step memory}}$ = 0.15 ± 0.01 CI, h = 0.49; including noise [SD = 0.4–5.0]: $M_{\text{impression-based}}$ = 0.26 ± 0.00 CI, $M_{\text{1-step memory}}$ = 0.10 ± 0.00 CI, h = 0.45). Before and after the onset of noise, non-contingent strategies dominated the population of winning strategies (without noise: $M_{\text{non-contingent}}$ = 0.50 ± 0.01 CI, $h_{\text{non-contingent\_1-step memory}}$ = 0.78, $h_{\text{non-contingent\_impression-based}}$ = 0.29; including noise: $M_{\text{non-contingent}}$ = 0.64 ± 0.01 CI, $h_{\text{non-contingent\_1-step memory}}$ = 1.23, $h_{\text{non-contingent\_impression-based}}$ = 0.79). Among the seven best strategies, there were five non-contingent strategies, of which two vanished with the onset of noise (D_1st_1–25C, D_1st_26–50C), because they cannot be represented with the chromosome we used without becoming noise-prone. After the D_1st-strategies vanished, probDef rapidly increased until reaching a plateau at around 51% of runs won. From SD = 1.6 on, probDef declined and another defecting non-contingent strategy (C_1–25) increased, taking over at SD = 3.8 and winning more of the 10,000 runs than any other individual strategy (see also Appendix Figure C1).

The seven best strategies mainly defect, resulting in low cooperation rates without and with small levels of noise. The D_1st-strategies (D_1st_1–25C, D_1st_26–50C) defect throughout except for the first interaction where they cooperate with less than 26% ($M_{\text{cooperation rate}}$ = 1.3, SD = 1.1, range = 0.1–3.2, Mdn = 1.3, Mo = 0.7) and 51% respectively ($M_{\text{cooperation rate}}$ = 3.7, SD = 3.5, range = 2.0–5.5, Mdn = 3.7, Mo = 3.4). Without noise, probGrim (M_{ind} = 45.4, SD = 45.2, range = 1–100, Mdn = 42, Mo = 1) and iGrim_Adding (M_{ind} = 47.5, SD = 47.3, range = 1–100, Mdn = 47, Mo = 10) more likely began with defection. Regardless of their partners' behaviour, they stuck to D (except for 10.0/7.4/0.8% of the cases with dc = 0/dd = 0/[dc = 0 and dd = 0] for probGrim and 9.4/10.8/0.6% for iGrim_Adding when there was an ind% probability to cooperate). If they cooperated, they punished exploitation (CD) with defection. Even in the case of mutual cooperation, continuous cooperation was not guaranteed (with cc = 0 [probGrim: in 30.3%, iGrim_Adding: in 18.3%] and ind < 100%). ProbDef, without noise, began more likely with defection (M_{ind} = 28.3, *SD* = 28.1, range = 1–100, *Mdn* = 20, *Mo* = 1). Regardless of its partners' behaviour, it stuck to D (except for 60.2/44.4/4.7% of the cases with dc = 0/dd = 0/ [dc = 0 and dd = 0] when there was an ind% probability to cooperate). As soon as probDef cooperated, though (cc/cd < 0), it would play D for the rest of the encounter (with ada = 0) or until the end of the window size respectively (with ada = 1).

The increase of the cooperation rate despite mostly defecting dominating strategies can have two reasons: First, the rise of two non-contingent C_strategies (C_1–25 and C_26–49) that, however, only cooperate on every move with less than 26% (cooperation rate exemplary at the simulation with *SD* = 5.0: *M* = 11.8, *SD* = 6.8, range = 1.4–27.3, *Mdn* = 11, *Mo* = 1.9) and 50% respectively (cooperation rate exemplary at the simulation with *SD* = 5.0: *M* = 35.5, *SD* = 30.5, range = 25.2–50.2, *Mdn* = 34.4, *Mo* = 28.7); second, noise. For the three noise-prone strategies probGrim, iGrim_Adding, and probDef to increase their cooperation rates, the probability to cooperate when indifferent (gene ind) would have to increase. The ind parameters for probGrim (without noise: M_{ind} = 45.4 ± 1.1 CI and exemplary for noise at the simulation with *SD* = 5.0: M_{ind} = 39.5 ± 4.9 CI; *d* = 0.19) and iGrim_Adding (without noise: M_{ind} = 47.5 ± 1.2 CI, *SD* = 5.0: M_{ind} = 29.2 ± 3.3 CI; *d* = 0.61) decrease with increasing noise, though. For probDef (without noise: M_{ind} = 28.3 ± 1.0 CI, *SD* = 5.0: M_{ind} = 36.3 ± 1.4 CI; *d* = 0.31), ind increases, but given this ind, probDef's cooperation rate would not exceed 0.19 without noise. If the three strategy's cooperation rates, despite of low ind, increased, it would be due to noise. This is in fact what we found (see Figure 3.4 exemplary for probDef's cooperation rates).

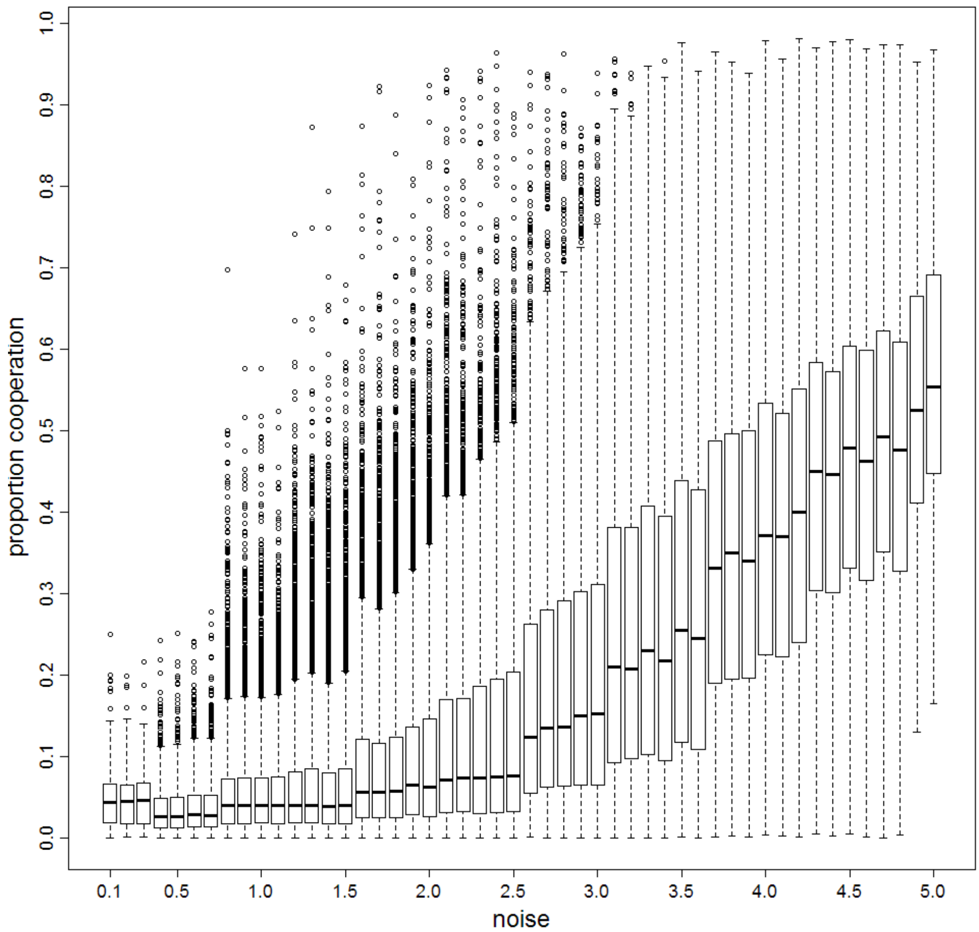

Figure 3.4. Cooperation rate (proportion of cooperation of all interactions) in the generation after a run stopped (i.e., all populations' chromosomes converted to one common pattern, one strategy) for all runs when probDef won. Boxplots show the median as a line inside the box, which contains 50% of the data (upper border = 75th percentile, lower border = 25th percentile). The whiskers range from 5 to 95% of the data, outliers are represented as circles.

The cross-validation

Cross-validation without noise

Our predictive accuracy measure of cross-validation sums the number of correct predictions and divides it by the number of cases in which the strategy makes a prediction over all of 20 partners, five cross-validation repetitions per participant, and over all gene constellations from the simulation results with *SD* = 0.1. Because they never won in the simulation and gene constellations were not available, we constructed parameter sets for Apologizer, probApologizer, Cooperator, Temptation, Shy, AntiTFT, probAntiTFT, Win-Change, Lose-Stay (WCLS), WSLC, Lurer, GreedyTFT, Sucker (all 1-step memory), and ExtremeAlternate (non-contingent). We omitted iApologizer_Averaging, iApologizer_ConAveraging, all iTemptation forms, iAntiTFT_ConAveraging for which there were no parameters from the simulation to cross-validate.

The predictive accuracy determines which strategy predicts a participant's behaviour best. We allowed ties so that several strategies can predict equally well. There were ties in 10 cases, resulting in 136 winner strategies with 126 participants. Counting the frequency each strategy was best-predicting, Figure 3.5 gives the order among the 136 best-predicting strategies. Impression-based strategies (iGrim_ConAdding, iGrim_ConAveraging, iSucker_ConAveraging, iShy_ConAveraging, iSucker_ConAdding, iShy_ConAdding) were more frequent than 1-step memory strategies (Shy, Temptation, TFT, probWCLS, AntiTFT, probTemptation, Grim; 24 versus 20). The most frequent best-predicting strategies, however, were non-contingent strategies like AllD, AllC, or variants thereof (with 92 best-predicted participants).

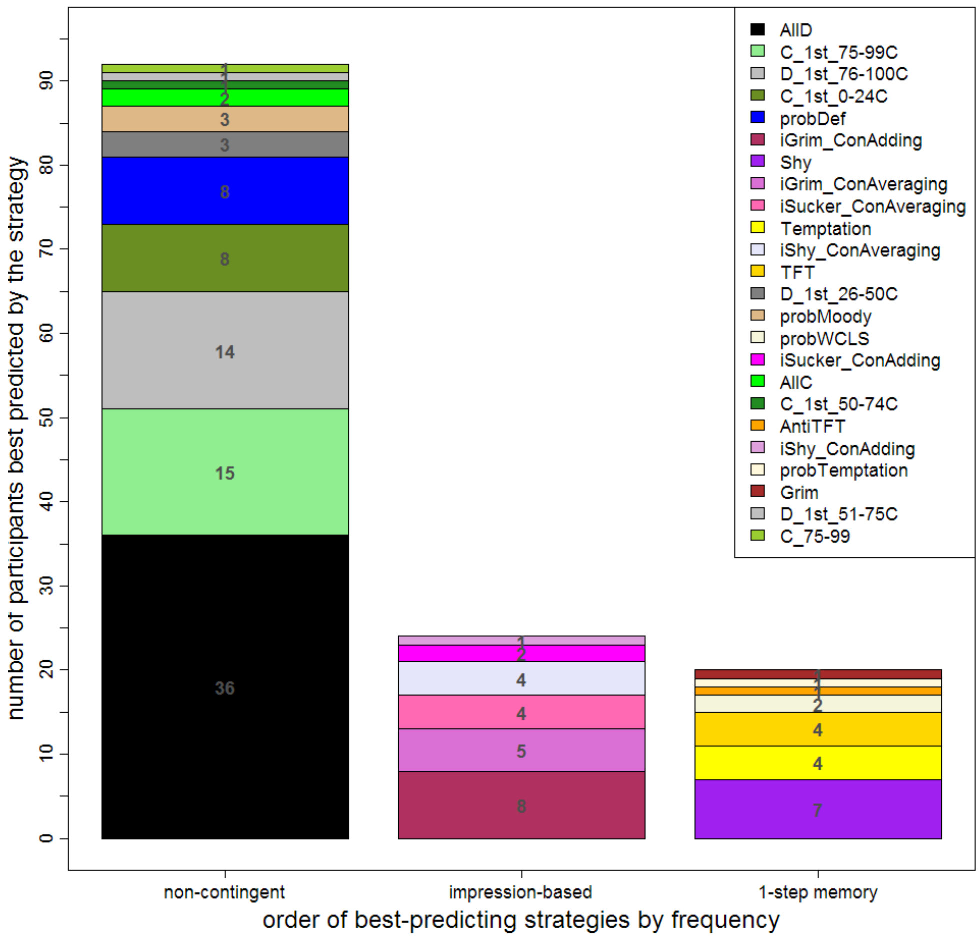

Figure 3.5. Order of best-predicting strategies (i.e., with highest predictive accuracy) by frequency for 126 participants. The legend displays the order of individual strategies independent of strategy group.

So far, we have only looked at the order of strategies by considering the frequency with which they had the highest predictive accuracy (number of correct predictions / number of predictions) per participant. The strategies, however, differ in predictive accuracy (Figure 3.6). For example, a strategy might have repeatedly best-predicted participants' behaviour among all strategies we tested (Figure 3.5), but if the quotient is only around 0.6, it is not a very accurate prediction.

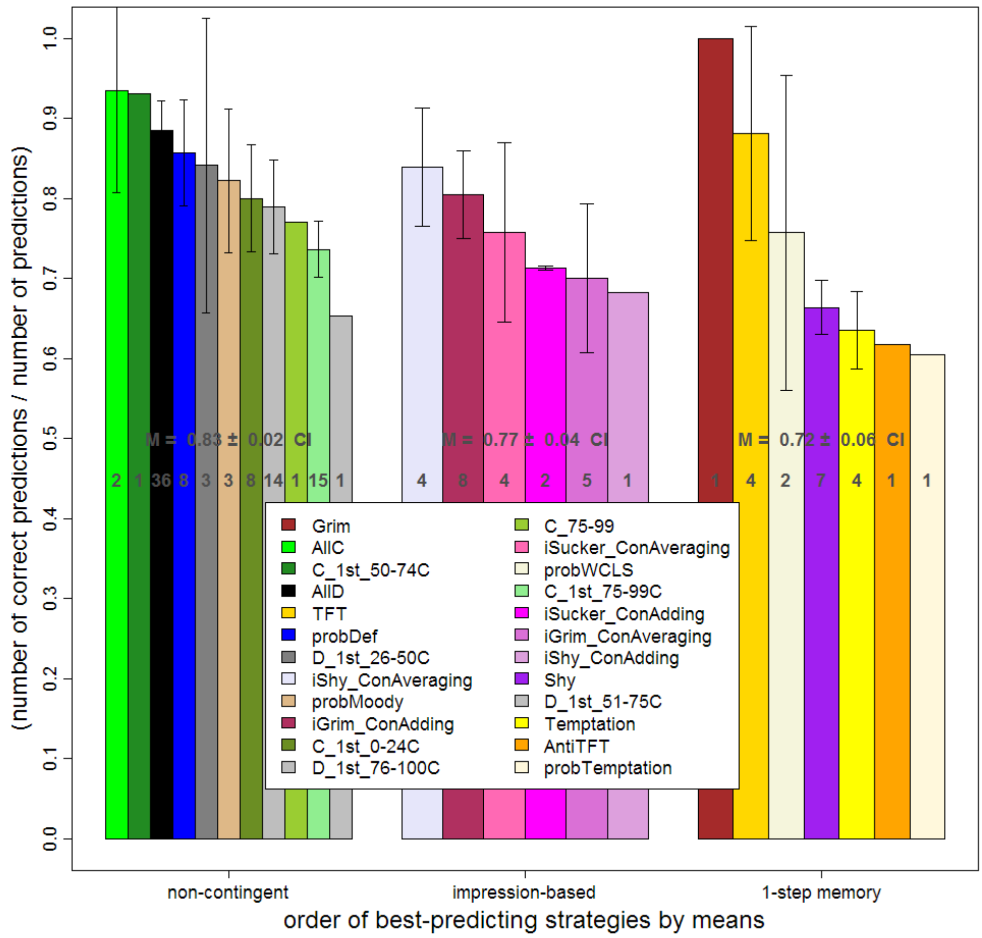

Figure 3.6. Order of best-predicting strategies by predictive accuracy of cross-validation (number of correct predictions / number of predictions). Numbers in the bars give the frequency of best-predicted participants by the corresponding strategy. Error bars are 95% confidence intervals and only depicted for strategies with frequency > 1. The legend displays the order of individual strategies independent of strategy group.

The predictive accuracy of the six impression-based strategies ($M_{\text{impression-based}}$ = 0.77 ± 0.04 CI) does not differ from that of the seven 1-step memory strategies ($M_{\text{1-step memory}}$ = 0.72 ± 0.06 CI). The 11 non-contingent strategies ($M_{\text{non-contingent}}$ = 0.83 ± 0.02 CI) predict participants' behaviour better than 1-step memory strategies (h = 0.26) and impression-based strategies (h = 0.16).

For a full picture of the best-predicting strategies' cross-validation performance, we multiplied the frequency each strategy was best predicting (as the proportion of all best-predicting strategies) with the strategy's mean predictive accuracy: combined measure =

(frequency of strategy / 136) $M_{\text{predictive accuracy}}$. This way, a rare strategy with high predictive accuracy can be outcompeted by a more frequent strategy with lower predictive accuracy. Figure 3.7 (left panel) shows the order of strategies by this combined measure.

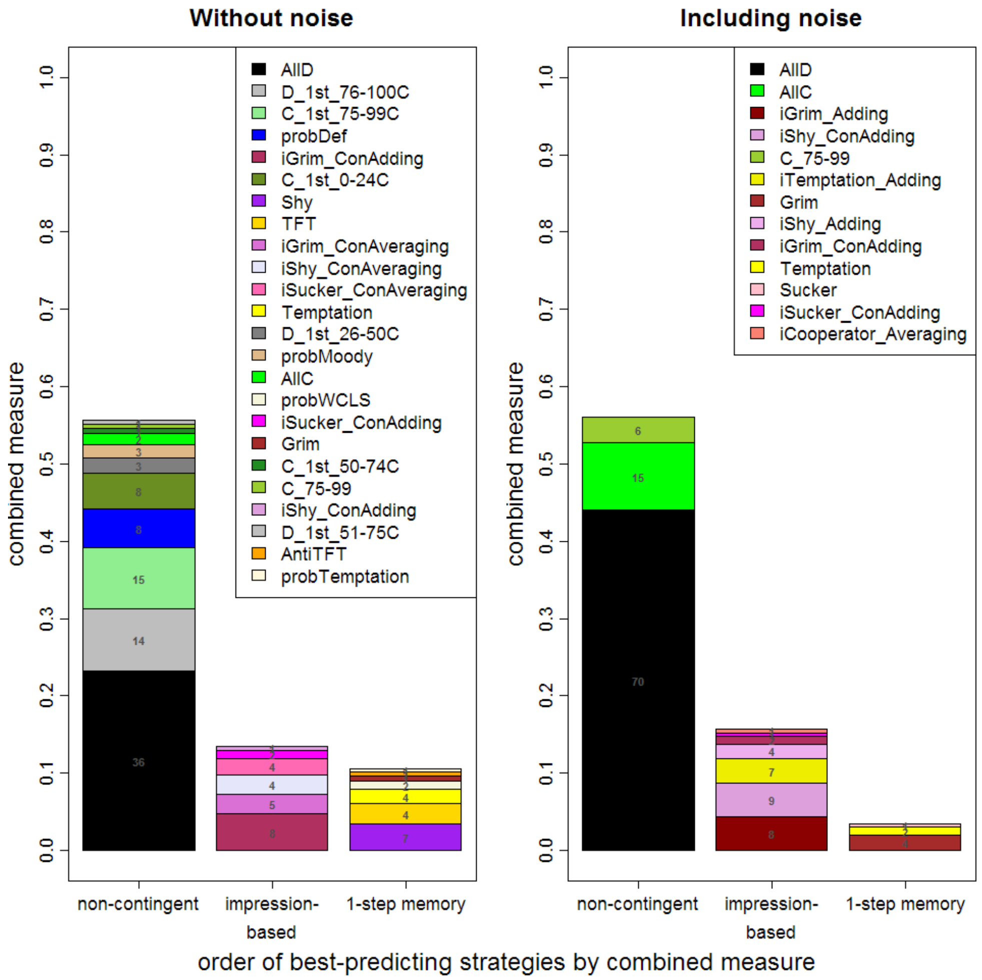

Figure 3.7. Order of combined measure of cross-validation by multiplying the strategy's proportion of all strategies and the strategy's predictive accuracy (number of correct predictions / number of predictions). Numbers in the bars give the frequency of best-predicted participants by the corresponding strategy. The left panel shows the results for cross-validation without noise and the right panel the results for cross-validation including noise. The legends display the order of individual strategies independent of strategy group.

The combined measure for the cross-validation of impression-based strategies ($M_{\text{impression-based}} = 0.14$) does not differ from that of 1-step memory strategies ($M_{\text{1-step memory}} = 0.11$). Non-contingent strategies ($M_{\text{non-contingent}} = 0.57$) have a higher combined cross-validation measure than 1-step memory ($h = 1.03$) and impression-based strategies ($h = 0.94$).

In sum, although more frequently best-predicting (Figure 3.5), impression-based strategies predict participants' behaviour equivalently to 1-step memory strategies (Figure 3.6) so that the combined measure does not differ (Figure 3.7, left panel). The most frequent and best-predicting strategies, however, are non-contingent strategies, above all AllD.

Cross-validation including noise

For the cross-validation including noise, the predictive accuracy sums the number of correct predictions and divides it by the number of cases in which the strategy makes a prediction over all of 20 partners, five cross-validation repetitions per participant, and over all gene constellations from the simulation results with *SD* = 5.0. Because they never won in the simulation and gene constellations were not available, we constructed parameter sets for Cooperator, Shy, WSLC, and TFT (all 1-step memory). We omitted D_1st_1–25C, D_1st_26–50C, D_1st_51–75C, D_1st_76–100C, C_1st_0–24C, C_1st_25–49C, C_1st_50–74C, C_1st_75–99C, and ExtremeAlternate which cannot be represented by the chromosome without becoming noise-prone.

There were ties in 4 cases, resulting in 130 winner strategies with 126 participants. Counting the frequency each strategy was best-predicting, impression-based strategies (iShy_ConAdding, iGrim_Adding, iTemptation_Adding, iShy_Adding, iGrim_ConAdding, iSucker_ConAdding, iCooperator_Averaging) were, as with cross-validation without noise, more frequent than 1-step memory strategies (Grim, Temptation, Sucker; 32 versus 7). The most frequent best-predicting strategies, however, were the non-contingent strategies AllD, AllC, and C_75–99 with 91 best-predicted participants.

As with the cross-validation without noise, the predictive accuracy of the seven impression-based strategies ($M_{\text{impression-based}}$ = 0.64 ± 0.02 CI) does not differ from that of the three 1-step memory strategies ($M_{\text{1-step memory}}$ = 0.64 ± 0.03 CI). The three non-contingent strategies ($\mathrm{M}_{\text{non-contingent}}$ = 0.80 ± 0.03 CI) predict participants' behaviour better than impression-based (*h* = 0.37) and 1-step memory strategies (*h* = 0.36).

For a full picture of the best-predicting strategies' cross-validation performance, we multiplied the frequency each strategy was best predicting (as the proportion of all best-predicting strategies) with the strategy's mean predictive accuracy: (frequency of strategy / 130) $M_{\text{predictive accuracy}}$. Figure 3.7 (right panel) shows the order of strategies by this combined measure. The combined measure for the cross-validation of impression-based strategies ($M_{\text{impression-based}}$ =

0.16) is, in contrast to the cross-validation without noise, higher than that of 1-step memory strategies ($M_{\text{1-step memory}}$ = 0.03; h = 0.44). As with cross-validation without noise, non-contingent strategies ($M_{\text{non-contingent}}$ = 0.56) have a higher combined cross-validation measure than 1-step memory (h = 1.32) and impression-based strategies (h = 0.88).

In sum, impression-based and 1-step memory strategies predict participants equally well, but because impression-based strategies more often predict participants' behaviour, the combined measure of impression-based strategies is higher than that of 1-step memory strategies (Figure 3.7, right panel). The most frequent and best-predicting strategies, however, are non-contingent strategies, above all AllD. Table 3.3 summarizes the cross-validation results.

Table 3.3

Summary of cross-validation results without and including noise as the order of strategies.

	cross-validation	
	without noise (using simulation parameters with *SD* = 0.1)	including noise (using parameters with *SD* = 5.0)
frequency with which participants are best predicted	1. non-contingent strategies 2. impression-based strategies 3. 1-step memory strategies	1. non-contingent strategies 2. impression-based strategies 3. 1-step memory strategies
quality with which participants are best predicted	1. non-contingent strategies 2. impression-based and 1-step memory strategies	1. non-contingent strategies 2. impression-based and 1-step memory strategies
combination of frequency and quality with which participants are best predicted	1. non-contingent strategies 2. impression-based and 1-step memory strategies	1. non-contingent strategies 2. impression-based strategies 3. 1-step memory strategies

Characteristics of the best-predicting impression-based strategies

Among impression-based strategies, the subforms iGrim and iShy dominate the best-predicting strategies without (see exemplary the first three ranks: iGrim_ConAdding: $M_{\text{combined measure}}$ = 0.05, iGrim_ConAveraging: $M_{\text{combined measure}}$ = 0.03, iShy_ConAveraging: $M_{\text{combined measure}}$ = 0.02) and including noise (iGrim_Adding: $M_{\text{combined measure}}$ = 0.04, iShy_ConAdding: $M_{\text{combined measure}}$ = 0.04, iTemptation_Adding: $M_{\text{combined measure}}$ = 0.03). IGrim and iShy are antipodes: Whereas iGrim, if it begins with cooperation, continues to cooperate and punishes exploitation (CD), iShy, if it begins with defection, continues to defect and reinforces cooperation (DC). Both forms have in common that if they begin with defection while the partner defects as well, they will continue to defect with a certain probability and, if so, do as well as the yardstick AllD. At the same time, for both, there are probabilities for mutual cooperation which yields more payoff than mutual defection and allows outcompeting AllD. On the other hand, the third-ranked iTemptation at the cross-validation including noise, cooperates after mutual defection but tries to exploit the partner after partner-sided cooperation (DC). One-step memory versions of iGrim, iShy, and iTemptation are also among the best-ranked 1-step memory strategies without (Shy: $M_{\text{combined measure}} = 0.03$, Temptation: $M_{\text{combined measure}} = 0.02$) and including noise (Grim: $M_{\text{combined measure}} = 0.02$, Temptation: $M_{\text{combined measure}} = 0.01$).

Discussion

We tested 1-step memory, impression-based, and non-contingent strategies competitively regarding their robustness to noise. To assess whether impression-based strategies are more robust to noise than 1-step memory strategies, we ran an evolutionary simulation using a genetic algorithm. There, agents, consisting of chromosomes that represent features of strategies, interacted in Iterated Prisoner's Dilemma games and spread in relation to their success in the population, supplemented by crossing over and mutation. We varied the level of noise in processing the partners' behaviour and found that, without and including noise, impression-based strategies outperformed 1-step memory strategies in maintaining cooperation and surviving. This confirms Hypothesis 1.

To test whether impression-based strategies are better able to predict human behaviour than 1-step memory strategies, we cross-validated the strategies on participants' behaviour from an earlier experiment. We found that, without noise, impression-based strategies result as best-predicting strategies more often but predict participants' behaviour about as well as 1-step memory strategies so that the combined measure is equivalent. Including noise, impression-based and 1-step memory strategies predict participants equally well, but impression-based strategies more often predict participants' behaviour so that the combined measure is higher. This confirms Hypothesis 2. Winner of the simulation and best in predicting participants' behaviour, though, are non-contingent strategies.

Reviewing the results

The simulation results surprised us in various ways. First, the population of chromosomes converted to one common pattern of gene parameters (i.e., strategy) already after a few generations ($M_{\text{generations}} = 78.2 \pm 0.1$ CI; not depicted). If one does not stop the simulation then, the fluctuations in the continuous genes (ind, cc–dd, ws) do not change the outcome much, whereas they are much more influential with the binary genes (ada, rec). Letting the simulation run until 1 Mio generations never fixes the population with respect to these two genes. Lindgren (1991), too, reported a constant change between stasis and unstable dynamic behaviour in the evolution of genes over the generations of one run.

This refers to the second observation: There was never just one winner over the 10,000 runs (i.e., 100% of runs won). Stopping the simulation after the first stabilization and repeating this procedure lead to an abundance of strategies to which the populations convert—especially before the onset of noise. Even from *SD* = 0.4 on and with probDef and, later, C_1–25 dominating the population, there were few other strategies present. Studies observing simulations with varying noise over generations (Lomborg, 1996) or stopping them after a predefined number of generations (Miller, 1996, Sherratt & Roberts, 1999) did not specify whether the population had converged to one common strategy in the course of the generations. We, on the other hand, did not systematically (i.e., over all noise levels) investigate the generations after the population had converged. A combination of the methods could clarify whether, after a dynamic phase, the population again evolves towards the once converged state or whether a different strategy would have won each time. Still, it remains open whether a strategy can be declared winner, because it is the first the population converges to or because it leads repeatedly to stable states in the course of the generations. As we needed a strict criterion to run repeated simulations per noise level and over noise levels and one which is computationally feasible we chose “the first strategy the population converges to”.

Third, the cooperation rate in the generation after a run stops increased with increasing noise. Other studies with varying noise found the opposite result (Lomborg, 1996, Miller, 1996, Sherratt & Roberts, 1999, Stevens et al., 2011). Our implementation of noise as a normal distribution with varying standard deviations around the correct impression index (with ada = 0) or weight (with ada = 1) deviates from the definition as the “probability of doing the opposite move” (as in Donninger, 1986, Müller, 1987, Lindgren, 1991, Nowak & Sigmund, 1993, Wu & Axelrod, 1995, Lomborg, 1996, Miller, 1996, Sherratt & Roberts, 1999, Wakano & Yamamura, 2001, Hruschka & Henrich, 2006). That is because the concept of an impression index as the basis for deciding what to do (instead of the partner’s previous move) required rethinking. We decided against choosing from a uniform distribution over all possible reactions (as did Sherratt & Roberts, 1999). Instead, we opted for a normal distribution, much as Bendor (1987) and colleagues did when they added an error term with *M* = 0 and *SD* = 8 to the (continuous) benefit the player received from the partner (Bendor et al., 1991). Because cooperation in our simulation was not continuous but the impression of the partner was, we set the impression index/weight equal to the mean of the normal distribution.

That cooperation rates increase with increasing noise is astonishing, as all noise-prone strategies that face noise in form of a normal distribution around the correct impression index/weight have the same probabilities for indices/weights to the left and right of the correct index/weight—where left leads to decreasing and eventually negative indices/weights resulting in defection and right leads to increasing and eventually positive indices/weights resulting in cooperation. Before accepting this as another piece of evidence for the emergence of cooperation under noise, we have to admit that increasing noise starts the decline of noise-prone strategies (for probDef after an initial increase until *SD* = 1.6). Whereas without noise (at the simulation with *SD* = 0.1–0.3), 18 of the 77 noise-prone strategies had proportions of 10,000 runs won > 1% (7 with > 3%), including noise there are just 12 with runs won > 1% (3 with > 3%). Although noise increases cooperation in the population, this increase does not seem to be advantageous for noise-prone strategies. Their decline paves the way for noise-independent non-contingent strategies like C_1–25 and C_25–49. With a proportion of (7 / 84 =) 8% of all tested strategies at the simulation with *SD* = 5.0, noise-independent non-contingent strategies sum to > 40% of 10,000 runs won.

Regarding the cross-validation, we were surprised that in a large proportion of cases mostly defecting strategies best-predicted participants' behaviour (cross-validation without noise: AllD, D_1st_76–100C which cooperates on the first move with 76–100% and defects from then on; cross-validation including noise: AllD). Which strategy participants pursue could depend on the partners with which they interact. In the experiment from which we took the participants' behaviour, there were three between-subjects conditions that differed in the proportion of partner types: defectors rare (20% defectors, 80% cooperators), equal proportion (50% defectors, 50% cooperators), and cooperators rare (80% defectors, 20% cooperators). Because the proportions of partner types may influence how participants react, we consider the frequency of best-predicting strategies per condition. In fact, the number of participants best-predicted by AllD at the cross-validation without noise increased with increasing defectors in the condition (6 participants in the defectors-rare, 11 in the equal-proportion, and 19 in the cooperators-rare condition). The same was true for AllD at the cross-validation including noise (13 participants in the defectors-rare, 23 in the equal-proportion, and 34 in the cooperators-rare condition). Here, also the opposite trend showed: The number of participants best predicted by the (mostly) cooperating strategies AllC (12, 2, 1) and C_75–99 (4, 2, 0) decreased with increasing defectors in the condition.

Impression-based and 1-step memory strategies predicted participants' behaviour less well than non-contingent strategies. Still, at the cross-validation including noise, impression-based strategies better-predicted participants' behaviour than 1-step memory strategies. As with non-contingent strategies, those impression-based strategies best-predicted participants' behaviour that more likely defected: iGrim_Adding (at the simulation with $SD = 5.0$: $M_{ind} = 29.2$, $SD = 23.5$, range = 0–100, $Mdn = 24$, $Mo = 7$), iShy_ConAdding (at the simulation with $SD = 5.0$: $M_{ind} = 41.2$, $SD = 22.1$, range = 1–86, $Mdn = 36$, $Mo = 36$), and iTemptation_Adding (at the simulation with $SD = 5.0$: $M_{ind} = 25.2$, $SD = 21.0$, range = 0–88, $Mdn = 20$, $Mo = 5$) all more likely began with defection. After the first move, iGrim_Adding (except for 4.1/8.2/0% of the cases with dc = 0/dd = 0/[dc = 0 and dd = 0] when there was an ind% probability to cooperate) more likely stuck to D regardless of the partners' behaviour, iShy_Adding more likely stuck to D if the partner defected as well. It is symptomatic that the more vulnerable to exploitation the lower ranked was a strategy in predicting participants' behaviour: Whereas iGrim_Adding does not cooperate after DC or DD (except for some remote probabilities, see above), iShy_ConAdding cooperates after DC, and iTemptation_Adding even cooperates after DD.

Other authors, too, found that, given noise, more information from the common history between player and partner is advantageous in contrast to 1-step memory strategies (Bendor, 1987, Lindgren, 1991, Hruschka & Henrich, 2006, Anh et al., 2011). Anh et al. (2011) describe the advantage of the extended knowledge with: "Having a greater memory size allows longer-term mutual trusts/distrusts to be built, and hence enables better recognition of erroneous moves". Whereas previous studies manifested extended knowledge of the partner's behaviour in the window size of past moves, in our results, impression-based strategies with potentially long memory (Averaging/ConAveraging) only ranked last among seven strategies. Instead, impression-based strategies succeeded that combine their extended knowledge about the partner in a single index one has to keep in mind (Adding/ConAdding).

The impression-based strategies' advantage of being immune to outliers of a partner's general behaviour is more prominent with regular in contrast to contradictory families. There were exclusively contradictory families among the best impression-based strategies at the cross-validation without noise but three contradictory and four regular families among the best strategies including noise. Contradictory families' being more provocable and less forgiving than regular ones seem to be features less successful with noise.

Implications

One-step memory and impression-based strategies hardly played a role in the simulation. In contrast, non-contingent strategies dominated the population of winning strategies before and after the onset of noise. This has implications for the design of new strategies to explain the emergence of cooperation and questions the success of 1-step memory strategies in previous studies. We conclude from participants' behaviour on which we based the cross-validation that participants pursue even simpler strategies than impression-based and 1-step memory. They stick to either C or D for the most part (without noise: AllD, D_1st_26–50C, C_1st_75–99C; including noise: AllD, AllC, iGrim_Adding), only influenced by the partner insofar as it becomes more likely D the more defector partners they interact with (without and including noise; including noise the more defectors the less AllC, too). Thus, participants display ecological rationality (Todd & Gigerenzer, 2007) by adapting their strategy to the environment, that is, the majority of their partners' behaviour. Participants do not seem to pay too much attention to the individual partners' strategies, though, by reacting to their every move (1-step memory strategies) or even general behaviour (impression-based strategies). After all, is human behaviour, at least in our artificial laboratory situation, not best described by reciprocity (Hammerstein, 2003) or even contingency on the partner? It seems the guideline for future strategies should be "less is more" not only what concerns the requirements to memory but also to contingency (Gigerenzer et al., 1999).

As a way to find these strategies, we deem a genetic algorithm a good method, because the strategies' parameters result from success—not from the experimenter's choice. As long as the experimenter chooses the set of strategies to compete against each other, there remain doubts whether a winner in such a simulation cannot be beaten by a strategy that was not included in the set of strategies. Some authors pit strategies evolved by a genetic algorithm simulation against famous strategies from the history of the research field (like TFT, WSLC, AllD) that they spare mutation and crossover (Golbeck, 2002, Dyer, 2004, Scali, 2006, Brunauer et al., 2007). Their justification is to test the evolved strategies against the best known ones. Under which circumstances were the best known strategies the best, though? Maybe these strategies would have never succeeded when given the same probability to evolve in an evolutionary simulation—as our simulation results show where TFT, WSLC, and even AllD are not among the most successful strategies.

Even though our genetic algorithm allowed for three groups of strategies (1-step memory, impression-based, non-contingent) and many subforms, at cross-validation without noise, none of them won with an overwhelming combined measure. Strategies with good predictive accuracy were not very frequent (Figure 3.6). At cross-validation including noise, on the other hand, at least the highest ranking three strategies with respect to combined measure also succeeded at frequency and quality (not depicted). Still, the challenge will be to design a genetic algorithm with the potential for even more and better-predicting strategies.

Limitations

Our simulation has the following disadvantages. First, we applied noise to the chromosome, not just specific strategies, but decided that, in contrast to other work (Bendor, 1993, Anh et al., 2011), non-contingent strategies are not influenced by noise. This meant, though, that we had to discard some non-contingent strategies with the onset of noise (D_1st- and C_1st-strategies, ExtremeAlternate), because their categorizations would have become noise-prone.

Second, 1-step memory and impression-based strategies rely on both, the partner's and the own previous move, to determine the behaviour for the current encounter. Whereas our experimental setup excluded missing moves by the partner (i.e., a programmed computer strategy), the player (i.e., the participant) could miss his move (intentionally or unintentionally). If the player, for example, did not decide to cooperate or defect on the first move, he still saw the partner's first move. There is no way to know whether the player behaved unaffectedly in the official second encounter (which would have let us to treat it as the first move and compare it with gene ind) or already based his move on the partner's behaviour. For the latter case, the chromosome did not offer a way to value the player's move. Here, genes "nac"/"nad" would probably help by specifying what to do if the player's move was not available (NA), whereas the partner cooperated or defected on the previous move. In our implementation, Adding and ConAdding strategies take the impression index from the second to last move (with NA on the first move: impression index = 0) as the basis for the current move. Averaging and ConAveraging strategies ignore the encounter just like they do weight genes = 0.

A limitation regarding the data we used for the cross-validation is that we gained them from participants who knew they played against computer partners. The knowledge of interacting with a computer, compared to a human, partner (when, in fact, both partner types adhered to the same strategy) decreased cooperation rates in Abric's (1984) experiment. Sanfey et al. (2003) reported that participants rejected unfair offers from computer partners at a significantly lower rate than those from human partners which the authors attributed to a stronger emotional reaction to humans than computers.

Future research

What are possible improvements of the simulation? First, if one interprets noise as memory errors, the realization in our simulation amounts to a fixed memory error not taking into account the time or number of other interaction partners lying between two interactions with the same partner. An alternative would be a partner- or time-wise memory error leading to a regression to the threshold between impressions the longer back the last interaction was.

Second, in our simulation, we set the threshold between cooperation and defection at 0, determined with a probability how likely an agent cooperates, and kept the threshold constant. A modification could be to have the threshold flexible throughout the interactions. Where and how does the decision-maker set the threshold in the first place? There are some approaches in which an agent observes his partner or other partners in the environment before interacting with him (Braver & Barnett, 1976, Pollock & Dugatkin, 1992, Nowak & Sigmund, 1998).

This refers to a third aspect of our simulation's setup: In everyday life, there are not only isolated interactions with partners, but one is publicly visible towards third parties in terms of a reputation. Strategic reputation-building may play an additional role in how someone reacts and whom he helps (Engelmann & Fischbacher, 2009).

Our results indicate that participants might use other strategies than impression-based or 1-step memory. One approach to get at them is to broaden the categories. Different people can have different categories (Medin, Lynch, Coley & Atran, 1997)—not everyone has to distinguish "good" and "bad". Nevertheless, humans tend to categorize items in their environment in binary distinctions, and we think "good" and "bad" are suitable. By incorporating one's own move into the formation of the partner's impression, more categories could arise, for example, "naive" or "patient". This would require building an impression index for oneself parallel to the one for the partner, as done in image-scoring strategies (Nowak & Sigmund, 1998).

Another possibility is that humans adhere to different strategies depending on whether they know the partner (Aktipis, 2006, Hruschka & Henrich, 2006) or depending on the partner type. We did not differentiate the cross-validation for partner types and implicitly suggested that participants pursued one strategy for all partner types. It could, however, be that humans have a toolbox of strategies from which they choose depending on the environment (Gigerenzer & Todd, 1999).

Although we could confirm both hypotheses, impression-based strategies' performance was not impressive. Other strategies than impression-based and 1-step memory were more successful in the simulation and the cross-validation—namely non-contingent strategies. Participants seem to use even simpler strategies than relying on their partner's previous move or general impression. Future research, using the suggestions we made, has to find out which ones these could be.

General Discussion

The most famous strategy to promote and maintain cooperation, TFT, is not robust to noise in form of perception or decision errors. Although some of its 1-step memory relatives were promoted as noise-robust alternatives, they, too, require to remember each partner's previous move. In Chapter 1, my collaborators and I investigated whether this requirement is cognitively feasible for a boundedly rational decision maker and tested robustness to noise given experimentally derived noise levels. In Chapter 2, my collaborators and I explored the cognitively more feasible strategy of assigning partner types and tested whether participants adapt their strategy to the environment. In Chapter 3, my collaborator and I modelled the process of assigning partner types by impression building and tested its robustness to noise and its ability to predict behaviour.

Tit-For-Tat and its relatives are not cognitively feasible and noise-robust

The experiment in Chapter 1 was designed to meet the requirements of 1-step memory strategies by asking participants to recall their partners' previous move. That participants had high memory error rates of 10–24% (depending on the number of partners) confirmed that 1-step memory strategies make cognitively infeasible memory requirements. Also, the rates offered a more realistic noise level than many of the theoretical studies had used before. This questions the generalizability of noise-robustness results by studies with noise below $p = 0.1$ to implement the opposite move. A minority of the studies I reviewed considered noise above $p = 0.1$ (Lomborg, 1996: $p = 0.00–0.125$, Wakano & Yamamura, 2001: $p = 0.01–0.2$, Hruschka & Henrich, 2006: $p = 0.0–0.2$, Anh et al., 2011: $p = 0.01–0.15$). Exclusively theoretical studies dealt with noise above $p = 0.2$ (Molander, 1985, Boyd, 1989, Bendor, 1993). At the noise level shown by the participants, 1-step memory strategies were almost non-existent in Chapter 1's simulations with 1-step memory and non-contingent strategies. Cooperation had reached a low point and eventually vanished with increasing noise, while AllD took over. If humans, in everyday life, adhered to the 1-step memory and non-contingent strategies as used in the simulations, cooperation would not exist in the world. Apparently, humans use different strategies to maintain cooperation in the face of noise. One possible strategy to decrease memory requirements was the subject of Chapter 2.

The strategy of assigning partner types is ecologically rational

In Chapter 2, my collaborators and I investigated whether participants categorize partners into types and preferentially remember one type, thereby reducing memory requirements. Asking participants about their partners several minutes and one week after interactions with them, they accurately determined partner types. Thus, participants noticed the difference in their partners' behaviour and assigned types accordingly. Categorization accuracy in general was very high (higher than in many other studies), but participants categorized the rare type in the environment more accurately (corrected for the chance level) than the common one. Presumably, participants remembered the rare and inferred the common type. That does not mean that they neglected partners of the common type, though. In this case, they would not have been able to distinguish partners of the inferred type from new partners in the recognition task. In sum, participants achieve the best with minimal effort and in the face of noise: To not get exploited by defectors but also not miss cooperation with cooperators, participants seemed to memorize their partners' faces, assign them to types based on the demonstrated behaviour, and preferentially remember the rare type in the environment to infer the common one. How humans could categorize partners into types, my collaborator and I explored in Chapter 3.

Impression-based strategies are cognitively feasible and noise-robust alternatives to 1-step memory strategies

Chapter 3 explored a strategy group proposed to model the process of partner type categorization. Building an impression of one's interaction partner instead of relying on the previous move turned out to be more noise-robust in a simulation with non-contingent, 1-step memory, and impression-based strategies. Moreover, impression-based strategies predicted participants' behaviour better than 1-step memory strategies, implying a better cognitive feasibility compared to remembering each partner's previous move. Most successful in the simulation and best-predicting in terms of participants' behaviour were non-contingent strategies, though. Apparently, previous research underestimated the influence of noise on the choice of interaction strategies in humans. Future research on cognitively feasible noise-robust strategies has to think in simpler dimensions. That does not imply going back to 1-step memory strategies but to strategies that probably do not rely too much on their partners' individual strategies. These strategies seem to stick mostly to cooperating or defecting after the initial acquainting and depending on the degree of hostility in the interaction group. A strategy (almost) not relying on noise-susceptible information is, of course, the best recipe against noise. Before throwing reciprocity overboard, one has to consider that the design of the simulation is improvable. Also, non-contingent strategies predicted participants' behaviour better than impression-based and 1-step memory strategies but in no way perfectly. The question which strategies humans use under noise to maintain cooperation remains to be answered.

The overall picture

The Prisoner's Dilemma is the dominant paradigm to explore the emergence of cooperation. Since its first formalisation in the early 1950's, and boosted by Axelrod's (1984) seminal work, authors have proposed ways to bring it closer to modelling the real world. Of the many innovations, I only mention: n-person Prisoner's Dilemma (e.g., Boyd & Richerson, 1988), stochasticity (e.g., Nowak & Sigmund, 1989, 1990, 1992), reputation (e.g., Kandori, 1992), spatial proximity (e.g., Nowak & May, 1992), continuous moves (e.g., Frean, 1996, Sherratt & Roberts, 1999), indirect reciprocity/image scoring (e.g., Nowak & Sigmund, 1998), be able to opt out of an interaction (e.g., McNamara, Barta & Houston, 2004), composition of the strategy environment (e.g., Aktipis, 2006), friendships (e.g., Hruschka & Henrich, 2006), or a combination of these (e.g., continuous cooperation and possibility to opt out: Sherratt & Roberts, 1998, continuous cooperation and reputation: Chong & Yao, 2007, continuous cooperation, spatial proximity, and variable memory: Alonso-Sanz, 2009). In three chapters, my collaborators and I investigated another branch: noise. The first chapter showed that the requirements of seemingly simple 1-step memory strategies push boundedly rational players to cognitive limits manifested in committing errors—and thus cannot be deemed cognitively feasible and noise-robust. The second chapter investigated a way of reducing memory requirements, thereby becoming more noise-robust, by assigning types to interaction partners and preferentially remembering one type over the other in an ecologically rational manner. In the third chapter, my collaborator and I designed strategies to investigate the way a partner type or impression could be built which were more noise-robust than 1-step memory strategies and predicted participants' behaviour better.

Besides noise as the overarching theme in my thesis, my collaborators and I included various other realistic elements. First, instead of meeting their interaction partners one (repeatedly) after the other, in the experiments in Chapters 1 and 2, participants met their partners repeatedly but randomly. Second, for testing partner type memory in Chapter 2, participants did not see predetermined labels but experienced partners repeatedly so that they could categorize them on their own. Third, to find the best of a number of strategies independent of the choice of the experimenter, my collaborator and I let it evolve from a rich set of parameters using a genetic algorithm simulation in Chapter 3.

Not all of the innovations proposed for the Prisoner's Dilemma embrace the pursuit of reality, however. Twenty years after Axelrod (1984), another tournament took place, aiming to find the best strategy for the Iterated Prisoner's Dilemma and, again, some of the submitted strategies entailed complex mathematical mechanisms (e.g., Au & Nau, 2006). Future research on the Iterated Prisoner's Dilemma should not lose sight of its original purpose which was to serve as a simplified version of human interactions with strategies supposed to model human behaviour. Mathematically elaborate strategies might be successful in the artificial laboratory situation, but do they capture human behaviour? The experiment in Chapter 1 demonstrated the cognitive infeasibility of 1-step memory strategies that were assumed to be simple rules. Chapter 3's cross-validation revealed that the majority of strategies failed to predict participants' behaviour.

Testing whether strategies comply with human behaviour has been widely neglected in the Iterated Prisoner's Dilemma literature. Although some authors compared participants' behaviour with strategies (Rapoport & Chammah, 1965, Oskamp, 1971, Wilson, 1971, Opp, 1988), these were results averaged over all participants. For reliable predictions about human behaviour, individual analyses are better suited (Gigerenzer & Brighton, 2009). The only individual-based cross-validations I found were from Wedekind and Milinski (1996, Milinski & Wedekind, 1998), but the authors did not specify their cross-validation method and participants' behaviour was categorized as either of two strategies based on which strategy made fewer mistakes in prediction. It would have been interesting to see for how many participants accuracy of prediction was below chance.

Although the Prisoner's Dilemma has been the dominant paradigm to investigate the emergence of cooperation, it has a limited scope. All innovations to make it more realistic cannot free it completely from a certain artificiality. Among others, Koenig (1988), Lazarus and Metcalfe (1990), and Stephens et al. (1995) list several cases in which a suspected Prisoner's

Dilemma situation in reality was later refuted. The latter authors close by warning that "Advocates of the Prisoner's Dilemma urgently need to articulate the empirical basis for continued interest in this research program".

Conclusion

The three chapters of this thesis demonstrated the importance of noise for the implementation of decisions: First, participants cannot use 1-step memory strategies without committing errors—and given these errors cooperation vanishes. Second, as a strategy to reduce memory requirements, participants assign partner types and adapt the type memory to their environment. Third, impression-based strategies are more noise-robust and better predict participants' behaviour than 1-step memory strategies. A strategy is best prepared against noise, however, if it mostly acts independently of the partner's behaviour. Investigating strategies in the Iterated Prisoner's Dilemma, my collaborators and I used methods incorporating reality: among others repeated but random interactions, a genetic algorithm simulation, and cross-validation. I argue that future research has to regard the capabilities of a boundedly rational decision maker. For an even better fit with everyday cooperative interactions, one might consider an alternative to the Iterated Prisoner's Dilemma game. Taking all of these considerations into account, research will come one step closer to finding cognitively feasible and noise-robust strategies explaining the emergence of cooperation.

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Appendix A

Document A1. Participant Instructions

Below is a translation from German of the participant instructions.

Instructions

In this experiment, you will repeatedly interact with a number of hypothetical partners. For each interaction, your partner will choose either to *cooperate* or *not cooperate*. **Your task is to recall the last action for each partner**.

To give you a concrete example of what this might mean, imagine that you repeatedly go out to dinner with each partner. At the end of the meal, you each must decide *individually* whether to contribute to a tip for the waiter. If your partner tips, this would be an instance of cooperating, but if your partner does not contribute to the tip, then this is not cooperating.

In this task, we will assess how well you remember whether each partner cooperated or not *the last time you interacted*.

Procedure

First you will be shown for each partner whether he/she cooperates or not. You should try to remember each partner's action. In the example below, Natalie cooperates.

Natalie kooperiert.

After observing all of the partners' actions one after the other, it follows the retrieval of the actions of the individual partners. For this purpose you will meet each partner again but not necessarily in the same order as in the beginning. Each time you will be asked whether the displayed partner cooperated or not the last time that you interacted with him/her.

Press ‘k’ for ‘cooperate’ or ‘n’ for ‘not cooperate’. You will have ten seconds to respond. If you wait longer than ten seconds, the question will be skipped.

After each response, you will learn whether you were correct. Thereafter you will see what the partner decides to do this time. In the example below, Natalie doesn’t cooperate this time.

This is now the action that you should try to keep in mind. **The task always is to recall the last action for the partner**. Then there will follow the retrieval, feedback, and new action for the next partner and so on.

Please respond as accurately as possible. You will receive 5 Cents for every correct response (in addition to your show-up fee of 5 Euro). Altogether you can receive an additional payment of 8 Euro on average. For incorrect responses or skipped questions, you will receive no payment.

Generally

For this experiment your partners will be grouped, such that you will repeatedly interact with the same partners before moving on to a new group of partners. Each group will have a different number of partners which you will interact with a different number of times. After you complete a group, you can have a short break before beginning the next group. The whole task should last about 1,5 hours.

You will begin with a practice phase in which you can see how the task works without earning money. If you have any questions, please ask the experimenter. If you are ready to begin the practice phase, please press <space bar> on the computer keyboard.

Document A2. Participant Questionnaire

Below is a translation from German of the participant questionnaire.

1. Do you know one/some of the depicted persons?
2. Did you associate memories of a/some certain person/s with one/some of the used names?
3. Of 10 decisions that you made how often did you guess on average?
4. Of 10 of your partner's actions how often, you think, did the interaction partners cooperate on average?
5. Did you pursue a certain strategy for memorizing the partner's actions? If you did, please describe the strategy you used.
6. What did you do when you couldn't remember the action from the previous round?
7. Do you have comments or suggestions?

Appendix B

Document B1. Instructions for the first session

This experiment is about social interactions. You will repeatedly interact with other people. Depending on what your interaction partner and you decide to do, you will receive points. These points will be converted into money which you will be paid in the end. Your interaction partners are not actually people, but they pursue strategies that have been identified in humans in experimental contexts before.

The interaction

The interaction is about agreeing with the opponent without being able to talk to one another. Imagine, for example, you produced some work in collaboration with a colleague. Your boss is not satisfied with the quality and calls the two of you individually into his office to search for reasons and maybe find the one to blame. Further, imagine your colleague and you just have the choice between “cooperating” or “refusing to cooperate” with the other. “Cooperating” in this case means to remain silent; “refusing” is to blame the other one. Even if your colleague was at your boss’ office first, you do not have a chance getting to know what he decided to do before you go in there yourself—you do not have the opportunity to talk to one another. Depending on what your colleague and you decide to do, there arise 4 possibilities:

1. You refuse to cooperate with your colleague and blame him, whereas he aims to cooperate and remains silent. Thereby, you are looking pretty good in the eyes of your boss; your colleague attracts the whole resentment.
2. Your colleague and you refuse to cooperate with each other and blame one another. The boss will think that none of you is completely innocent when it comes to the quality of the work and call both of you to account.
3. Your colleague and you cooperate and remain silent what concerns the one to blame. Your boss will be insecure and teach both of you at least a little lesson.
4. You protect your colleague and remain silent, whereas he blames you. You will have to carry the whole damage yourself, whereas your colleague gets away without a penalty.

In the experiment, these different results are translated into points you earn, depending on what your partner and you decide to do. You see the distribution of points in Table 1.

Table 1

Payoff in points for all interaction situations.

You	Interaction Partner	
	Cooperate	Refuse
Cooperate	3 ; 3	0 ; 5
Refuse	5 ; 0	1 ; 1

Please have a close look at the payoff matrix: You cannot just see the points you will receive for all of the four situations (left value in each cell) but also those that your partner will get (right value in each cell). If you decide, for example, to "refuse", the other person to "cooperate", the lower left cell comes into effect: You receive 5 points, your interaction partner 0. If both of you "refuse", each one of you earns 1 point (lower right cell). If the two of you opt for "cooperate", each of you will get 3 points (upper left). If you choose "cooperate", your partner "refuse", you will get 0 points, your partner 5 (upper right).

The procedure

First, the partner will be introduced to you with image and name (Please note that the people on the images were asked to look neutrally. Jewellery and possible make-up were removed. They all wear the same t-shirt.). In the example below, your partner is Bernd.

You are meeting Bernd.

After that, you will be asked to choose one of two alternatives. To do so, please press the key "q" for "cooperate" or "p" for "refuse". You will have ten seconds to react. If you wait longer than ten seconds, the question will be skipped.

you	interaction partner	
	cooperate	refuse
cooperate	3 ; 3	0 ; 5
refuse	5 ; 0	1 ; 1

What do you decide to do with Bernd?

cooperate **refuse**

Assume you chose “cooperate”. Imagine your interaction partner (here: Bernd) got the question what to do with you at the same time. On the next screen (and in the example below) you will experience what the interaction partner decided to do.

Bernd refused.

Next, you will be shown with the help of the payoff matrix how many points you and your partner receive. In this case, you will get 0 points, Bernd 5.

you	interaction partner	
	cooperate	refuse
cooperate	3 ; 3	0 ; 5
refuse	5 ; 0	1 ; 1

You cooperated, Bernd refused.
You get 0 points, Bernd 5.

Therewith, the interaction with Bernd is over. What follows is introduction, decision question, partner decision, and payoff information for the next partner and so on. After you have interacted with each person, you will meet all of them again repeatedly and in random order.

A certain percent of the overall number of points you will be paid in the end (additionally to the 5 Euro show-up fee). All in all, you may earn approximately 4–16 Euro additionally. You will not earn money for skipped questions.

The overview

The experiment begins with a phase which will test whether you understood the payoff matrix. You will only continue when you answered more than 80% of the questions correctly. After that, there follows a practice phase in which you will get to know the interaction situation without earning money. Then, the actual interactions will take place. Thereupon, two tasks are attached for which instructions will be given on the screen. The whole session should take about 80 min.

As the experimenter already told you, this experiment requires an additional session (approximately 40 minutes) in a week. You absolutely have to participate in this; the date cannot be postponed. Details concerning the procedure and the tasks will be given to you then.

Should you have any questions, please ask the experimenter. If you are ready to begin with the first phase, please press the space bar on the keyboard.

Have fun with the experiment and thanks for your collaboration!

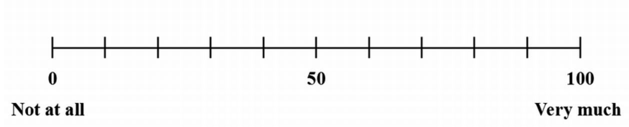
Document B2. Questionnaire

1. Did you pursue a certain strategy in interacting with the partners? If yes, please describe the strategy you used.

2. In case you memorized cooperators/refusers in particular: Why did you choose the one or the other group?

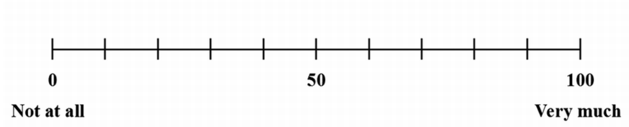
3. How did you come to a decision when you could not remember your partner's action from the last interaction?

4. How much were you involved emotionally in the experiment?



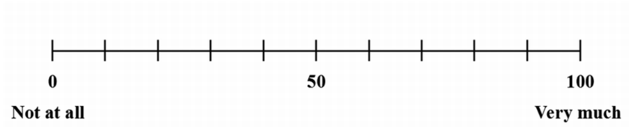
5. Were you angry about something? If yes, what was it?

6. How angry were you on a scale from 0 (*not at all*) until 100 (*very much*)?



7. Were you happy about something? If yes, what was it?

8. How happy were you on a scale from 0 (*not at all*) until 100 (*very much*)?

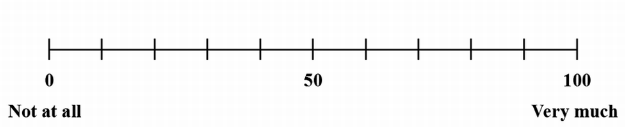


9. Do you know one/some of the depicted persons? If yes, which one/ones?

10. Did you associate the memory of a specific person/specific persons with one/some of the names? If yes, with which name/names?

11. Did you notice differences between the pictures (colour of the background/shirt, light, quality)? If yes, which pictures differed?

12. How interesting did you find the whole study (Session 1 and 2) on a scale from 0 (*not at all*) until 100 (*very much*)?



13. If you have comments or suggestions concerning the experiment, please note them.

Document B3. Categorization using Barclay's method

To compare our findings to Barclay's (2008), we applied his analytical method to our data. For accuracy rates, he considered the correct categorizations of the 20 old partners, independent of correct recognition (Figure B1a). For chance level, he proposed the perceived proportion of partner types among old and new partners. To correct the accuracy rates for the chance levels, Barclay used a relative difference [(accuracy rate - chance level) / chance level] and averaged across participants. We excluded data from 3 participants due to division by 0. Applying Barclay's method to our data, we found that, in the defectors-rare and equal-proportion condition, defectors were categorized more accurately than cooperators; in the cooperators-rare condition, cooperators were categorized more accurately than defectors (Figure B1b). When looking at individual participants, the majority of them showed the pattern in the respective condition: 97% of participants in the defectors-rare and 77% in the equal-proportion condition categorized defectors better than they categorized cooperators, 66% in the cooperators-rare condition categorized cooperators better than they categorized defectors.

We observed a similar pattern of results in the second session (Figures B1c, d). The individual data analysis showed that this held for most of the participants: 83% in the defectors-rare condition and 61% in the equal condition categorized defectors better than they categorized cooperators, 66% in the cooperators-rare condition categorized cooperators better than they categorized defectors.

In both sessions, the results, given the chance levels of perceived proportion of partner types among old and new partners, support the predictions of the rarity hypothesis in that participants better remember the partner type that is rare in the interaction group.

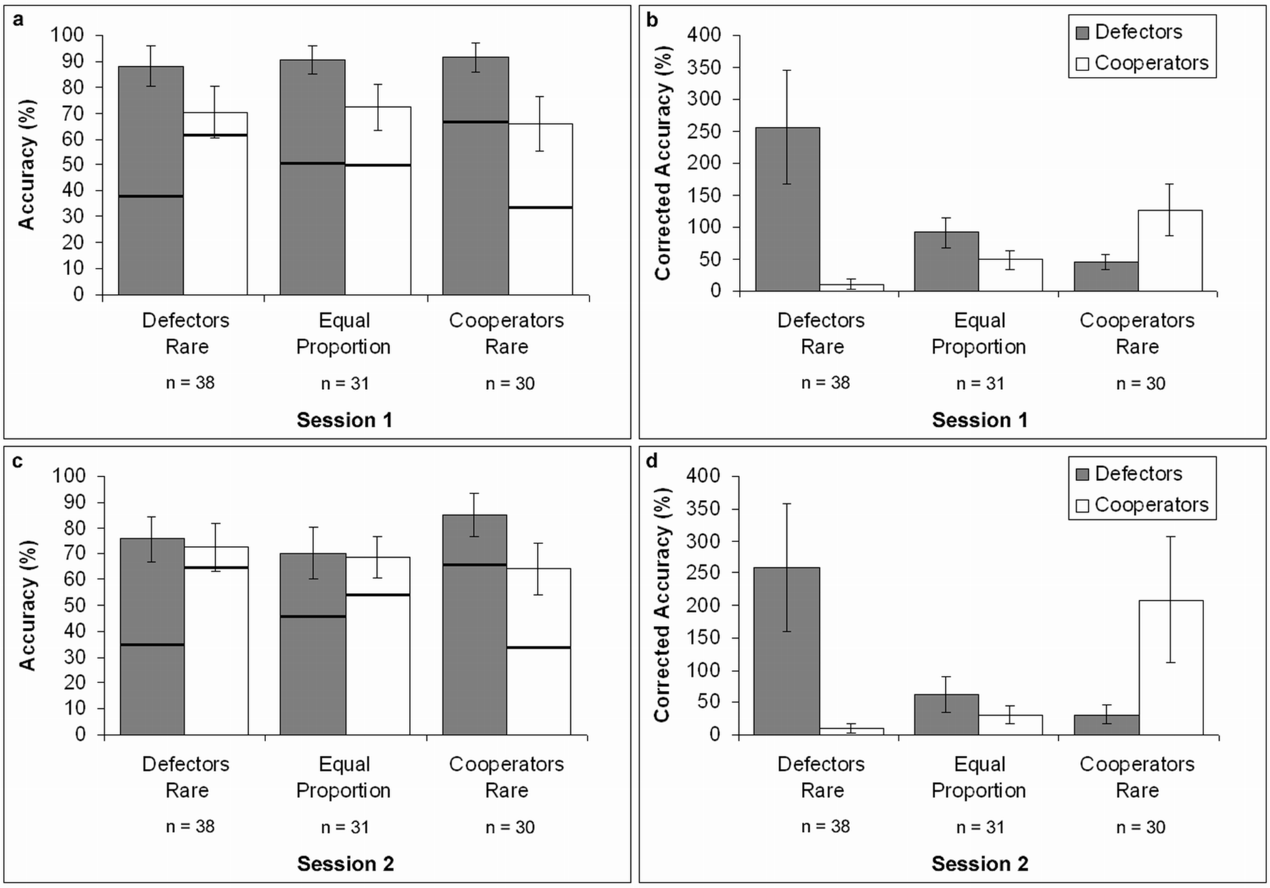


Figure B1. Accuracy rates using Barclay's method. Part (a) depicts the categorization accuracy for old partners independent of correct recognition (± 95% confidence interval) in the three conditions in the first session. The solid line represents the chance levels based on the perceived proportion of partner types among old and new partners. In part (b), we present the relative differences between accuracy rates and chance levels using [(accuracy rate - chance level) / chance level] averaged across participants. The lower parts (c and d) show the respective results from the second session after 1 week. In the cooperators-rare condition, we averaged across n = 30 for defectors and n = 29 for cooperators in each session.

Table B1

Accuracy rates for recognition, categorization independent of correct recognition, and categorization in conjunction with correct recognition (with 95% confidence intervals) from different studies investigating partner-type memory.

Study	Condition	Partner Type	Recognition	Categorization independent of correct recognition	Categorization in conjunction with correct recognition (SIM†)
Retention interval: 0–10 min					
Volstorf, Rieskamp & Stevens	Defectors Rare	Defectors	97.4 ± 3.1	88.2 ± 7.8	86.2 ± 8.8
		Cooperators	99.0 ± 0.7	70.3 ± 9.8	69.7 ± 9.9
	Equal Proportion	Defectors	99.7 ± 0.6	90.3 ± 5.6	90.0 ± 5.5
		Cooperators	100.0	72.2 ± 8.9	72.2 ± 8.9
	Cooperators Rare	Defectors	98.8 ± 1.2	91.5 ± 5.6	90.9 ± 5.7
		Cooperators	98.3 ± 2.3	65.8 ± 10.6	65.0 ± 10.4
Barclay (2008; personal communication)	Defectors Rare	Defectors	81.9 ± 7.4	38.7 ± 9.0	32.5 ± 8.8
		Cooperators	73.9 ± 5.7	80.9 ± 5.2	62.0 ± 6.9
	Equal Proportion	Defectors	70.3 ± 10.5	58.5 ± 9.0	44.3 ± 10.2
		Cooperators	75.7 ± 8.0	63.0 ± 7.6	51.8 ± 8.8
	Cooperators Rare	Defectors	75.8 ± 7.3	85.3 ± 4.1	64.8 ± 6.2
		Cooperators	85.0 ± 4.9	40.0 ± 12.9	35.6 ± 12.1
Chiappe et al. (2004)	Equal Proportion	Defectors	73.1 ± 0.4*	–	42.5 ± 0.5*
		Cooperators	68.8 ± 0.4*	–	35.6 ± 0.4*
Farrelly & Turnbull (2009)	Equal Proportion	Defectors	47.5–57.5 ± 0.2*‡	–	22.5–35.0 ± 0.2*‡
		Cooperators	42.5–47.5 ±	–	25.0–27.5 ±

			0.2*‡		0.2*‡
Retention interval: 1 week					
Volstorf, Rieskamp & Stevens	Defectors Rare	Defectors	98.7 ± 1.8	75.7 ± 8.8	75.0 ± 9.1
		Cooperators	98.4 ± 1.0	72.5 ± 9.4	71.5 ± 9.2
	Equal Proportion	Defectors	99.0 ± 1.1	70.0 ± 10.1	69.0 ± 10.1
		Cooperators	99.0 ± 1.1	68.7 ± 8.1	68.1 ± 8.1
	Cooperators Rare	Defectors	95.4 ± 3.0	85.0 ± 8.4	82.5 ± 8.7
		Cooperators	99.2 ± 1.6	64.2 ± 9.9	63.3 ± 9.9
Mealey et al. (1996)	Equal Proportion	Defectors	43.3–77.5‡	–	–
		Cooperators	40.0–46.7‡	–	–
Oda (1997)	Equal Proportion	Defectors	46.7–50.0 ± 0.4–0.5*‡	–	–
		Cooperators	34.4–51.1 ± 0.4–0.5*‡	–	–

Note. We only report partner-memory studies that provided raw values in the paper. * We calculated the 95% confidence interval from the standard deviations given. † SIM = Source Identification Measure. ‡ The values give the range of results for studies distinguishing several conditions or separating participants by gender.

Appendix C

Table C1

Strategies or strategy families in which we categorized the chromosome.

Strategy	Description and chromosome implementation
1-step memory strategies	
Apologizer	defect on the first move, if defected, cooperate, if cooperated and partner cooperated, defect, if cooperated and partner defected, cooperate $0, < 0, \geq 0, > 0, > 0, 0, *, 1$, with ($\lvert cc \rvert \geq dc$), ($\lvert cc \rvert \geq dd$), $0, < 0, > 0, > 0, > 0, 1, 1, *$, $0, < 0, > 0, > 0, > 0, 1, 2, *$, with ($\lvert cc \rvert \geq cd$), ($\lvert cc \rvert \geq dc$), ($\lvert cc \rvert \geq dd$)
probApologizer (probabilistic Apologizer)	defect on the first move with probability (100-ind), if defected, cooperate, if cooperated and partner cooperated, defect, if cooperated and partner defected, cooperate (at $cd = 0$ with probability ind) $*, < 0, \geq 0, > 0, > 0, 0, *, 1$, with ($\lvert cc \rvert \geq dc$), ($\lvert cc \rvert \geq dd$), $*, < 0, \geq 0, > 0, > 0, 1, 1, *$, $*, < 0, \geq 0, > 0, > 0, 1, 2, *$, with ($\lvert cc \rvert \geq cd$), ($\lvert cc \rvert \geq dc$), ($\lvert cc \rvert \geq dd$)
Cooperator	defect on the first move, if defected, cooperate, if cooperated and partner cooperated, cooperate, if cooperated and partner defected, defect $0, \geq 0, < 0, > 0, > 0, 0, *, 1$, with ($\lvert cd \rvert \geq dc$), ($\lvert cd \rvert \geq dd$), $0, > 0, < 0, > 0, > 0, 1, 1, *$, $0, > 0, < 0, > 0, > 0, 1, 2, *$, with ($\lvert cd \rvert \geq cc$), ($\lvert cd \rvert \geq dc$), ($\lvert cd \rvert \geq dd$)
probCooperator (probabilistic Cooperator)	defect on the first move with probability (100-ind), if defected, cooperate, if cooperated and partner cooperated, cooperate (at $cc = 0$ with probability ind), if cooperated and partner defected, defect $*, \geq 0, < 0, > 0, > 0, 0, *, 1$, with ($\lvert cd \rvert \geq dc$), ($\lvert cd \rvert \geq dd$), $*, \geq 0, < 0, > 0, > 0, 1, 1, *$, $*, \geq 0, < 0, > 0, > 0, 1, 2, *$, with ($\lvert cd \rvert \geq cc$), ($\lvert cd \rvert \geq dc$), ($\lvert cd \rvert \geq dd$)
Temptation	defect on the first move, if defected and partner cooperated, defect, if defected and partner defected, cooperate from then on, $0, \geq 0, \geq 0, \leq 0, > 0, 0, *, 1$, with ($dd > \lvert dc \rvert$), $0, > 0, > 0, \leq 0, > 0, 1, 1, *$, $0, > 0, > 0, \leq 0, > 0, [1, 2, 0] / [1, > 1, 1]$, with ($dd > \lvert dc \rvert$)

probTemptation
(probabilistic Temptation)

defect on the first move with probability (100-ind),
if defected and partner cooperated, defect (at dc = 0 with probability (100-ind)),
if cooperated, cooperate (at cc/cd = 0 with probability ind),
if defected and partner defected, cooperate from then on
*, ≥ 0, ≥ 0, ≤ 0, > 0, 0, *, 1, with (dd > |dc|),
*, ≥ 0, ≥ 0, ≤ 0, > 0, 1, 1, *,
*, ≥ 0, ≥ 0, ≤ 0, > 0, [1, 2, 0] / [1, > 1, 1], with (dd > |dc|)

Shy

defect on the first move,
if defected and partner defected, defect,
if defected and partner cooperated, cooperate from then on
0, ≥ 0, ≥ 0, > 0, ≤ 0, 0, *, 1, with (dc > |dd|),
0, > 0, > 0, > 0, ≤ 0, 1, 1, *,
0, > 0, > 0, > 0, ≤ 0, [1, 2, 0] / [1, > 1, 1], with (dc > |dd|)

probShy
(probabilistic Shy)

defect on the first move with probability (100-ind),
if defected and partner defected, defect (at dd = 0 with probability (100-ind)),
if cooperated, cooperate (at cc/cd = 0 with probability ind),
if defected and partner cooperated, cooperate from then on
*, ≥ 0, ≥ 0, > 0, ≤ 0, 0, *, 1, with (dc > |dd|),
*, ≥ 0, ≥ 0, > 0, ≤ 0, 1, 1, *
*, ≥ 0, ≥ 0, > 0, ≤ 0, [1, 2, 0] / [1, > 1, 1], with (dc > |dd|)

AntiTFT
(Anti-Tit-For-Tat)

defect on the first move,
if partner cooperated, defect,
if partner defected, cooperate
0, < 0, ≥ 0, ≤ 0, > 0, 0, * , 1, with (|cc| = dd), (dd > |dc|)
0, < 0, > 0, ≤ 0, > 0, 1, 1, *
0, < 0, > 0, ≤ 0, > 0, 1, 2, *, with (dd > |dc|), (|cc| ≥ cd), (|cc| ≥ dd)

probAntiTFT
(probabilistic
Anti-Tit-For-Tat)

defect on the first move with probability (100-ind),
if partner cooperated, defect (at dc = 0 with probability (100-ind)),
if partner defected, cooperate (at cd = 0 with probability ind)
*, < 0, ≥ 0, ≤ 0, > 0, 0, *, 1, with (|cc| = dd), (dd > |dc|),
*, < 0, ≥ 0, ≤ 0, > 0, 1, 1, *
*, < 0, ≥ 0, ≤ 0, > 0, 1, 2, *, with (dd > |dc|), (|cc| ≥ cd), (|cc| ≥ dd)

WCLS
(Win-Change, Lose-Stay)

defect on the first move,
if defected and partner cooperated, cooperate,
if defected and partner defected, defect,
if cooperated and partner cooperated, defect,
if cooperated and partner defected, cooperate
0, < 0, ≥ 0, > 0, ≤ 0, 0, *, 1, with (|cc| ≥ dc), (dc > |dd|),
0, < 0, > 0, > 0, ≤ 0, 1, 1, *,
0, < 0, > 0, > 0, ≤ 0, 1, 2, *, with (|cc| ≥ dc), (|cc| ≥ cd), (dc > |dd|)

probWCLS (probabilistic

defect on the first move with probability (100-ind), Win-Change, Lose-Stay)

if defected and partner cooperated, cooperate,
if defected and partner defected, defect (at $dd = 0$ with probability (100-ind)),
if cooperated and partner cooperated, defect,
if cooperated and partner defected, cooperate (at $cd = 0$ with probability ind)
$*, < 0, \geq 0, > 0, \leq 0, 0, *, 1$, with ($|cc| \geq dc$), ($dc > |dd|$),
$*, < 0, \geq 0, > 0, \leq 0, 1, 1, *$,
$*, < 0, \geq 0, > 0, \leq 0, 1, 2, *$, with ($|cc| \geq dc$), ($|cc| \geq cd$), ($dc > |dd|$)

WSLC
(Win-Stay, Lose-Change)

cooperate on the first move,
if cooperated and partner cooperated, cooperate,
if cooperated and partner defected, defect,
if defected and partner cooperated, defect
if defected and partner defected, cooperate
$100, \geq 0, < 0, \leq 0, > 0, 0, *, 1$, with ($|cd| > cc$), ($|cd| = dd$),
$100, \geq 0, < 0, < 0, > 0, 1, 1, *$,
$100, \geq 0, < 0, < 0, > 0, 1, 2, *$, with ($|cd| > cc$), ($dd \geq |cd|$), ($dd \geq |dc|$)

probWSLC (probabilistic
Win-Stay, Lose-Change)

cooperate on the first move with probability ind,
if cooperated and partner cooperated, cooperate (at $cc = 0$ with probability ind),
if cooperated and partner defected, defect
if defected and partner cooperated, defect (at $dc = 0$ with probability (100-ind)),
if defected and partner defected, cooperate
$*, \geq 0, < 0, \leq 0, > 0, 0, *, 1$, with ($|cd| > cc$), ($|cd| = dd$),
$*, \geq 0, < 0, \leq 0, > 0, 1, 1, *$,
$*, \geq 0, < 0, \leq 0, > 0, 1, 2, *$, with ($|cd| > cc$), ($dd \geq |cd|$), ($dd \geq |dc|$)

TFT (Tit-For-Tat)

cooperate on the first move,
if partner cooperated, cooperate,
if partner defected, defect
$100, \geq 0, < 0, > 0, \leq 0, 0, *, 1$, with ($|cd| > cc$), ($|cd| = dc$),
$100, \geq 0, < 0, > 0, < 0, 1, 1, *$,
$100, \geq 0, < 0, > 0, < 0, 1, 2, *$, with ($|cd| > cc$), ($dc \geq |cd|$), ($dc \geq |dd|$)

probTFT (probabilistic
Tit-For-Tat)

cooperate on the first move with probability ind,
if partner cooperated, cooperate (at $cc = 0$ with probability ind),
if partner defected, defect (at $dd = 0$ with probability (100-ind))
$*, \geq 0, < 0, > 0, \leq 0, 0, *, 1$, with ($|cd| > cc$), ($|cd| = dc$),
$*, \geq 0, < 0, > 0, \leq 0, 1, 1, *$,
$*, \geq 0, < 0, > 0, \leq 0, 1, 2, *$, with ($|cd| > cc$), ($dc \geq |cd|$), ($dc \geq |dd|$)

Lurer

cooperate on the first move,
if cooperated, defect,
if defected and partner cooperated, defect,
if defected and partner defected, cooperate

	100, < 0, < 0, ≤ 0, > 0, 0, *, 1, with (dd ≥ |cc|), (dd ≥ |cd|), 100, < 0, < 0, < 0, > 0, 1, 1, *, 100, < 0, < 0, < 0, > 0, 1, 2, *, with (dd ≥ |cc|), (dd ≥ |cd|), (dd ≥ |dc|)
probLurer (probabilistic Lurer)	cooperate on the first move with probability ind, if cooperated, defect, if defected and partner cooperated, defect (at dc = 0 with probability (100-ind)), if defected and partner defected, cooperate *, < 0, < 0, ≤ 0, > 0, 0, *, 1, with (dd ≥ |cc|), (dd ≥ |cd|), *, < 0, < 0, ≤ 0, > 0, 1, 1, *, *, < 0, < 0, ≤ 0, > 0, 1, 2, *, with (dd ≥ |cc|), (dd ≥ |cd|), (dd ≥ |dc|)
GreedyTFT (Greedy Tit-For-Tat)	cooperate on the first move, if cooperated, defect, if defected and partner cooperated, cooperate, if defected and partner defected, defect 100, < 0, < 0, > 0, ≤ 0, 0, *, 1, with (dc ≥ |cc|), (dc ≥ |cd|), 100, < 0, < 0, > 0, < 0, 1, 1, *, 100, < 0, < 0, > 0, < 0, 1, 2, *, with (dc ≥ |cc|), (dc ≥ |cd|), (dc ≥ |dd|)
probGreedyTFT (probabilistic Greedy Tit-For-Tat)	cooperate on the first move with probability ind, if cooperated, defect, if defected and partner cooperated, cooperate, if defected and partner defected, defect (at dd = 0 with probability (100-ind)) *, < 0, < 0, > 0, ≤ 0, 0, *, 1, with (dc ≥ |cc|), (dc ≥ |cd|), *, < 0, < 0, > 0, ≤ 0, 1, 1, *, *, < 0, < 0, > 0, ≤ 0, 1, 2, *, with (dc ≥ |cc|), (dc ≥ |cd|), (dc ≥ |dd|)
Sucker	cooperate on the first move, if cooperated and partner defected, cooperate, if cooperated and partner cooperated, defect from then on 100, < 0, ≥ 0, ≤ 0, ≤ 0, 0, *, 1, with (|cc| > cd), 100, < 0, ≥ 0, < 0, < 0, 1, 1, * 100, < 0, ≥ 0, < 0, < 0, [1, 2, 0] / [1, > 1, 1], with (|cc| > cd)
probSucker (probabilistic Sucker)	cooperate on the first move with probability ind, if defected, defect (at dc/dd = 0 with probability (100-ind)), if cooperated and partner defected, cooperate (at cd = 0 with probability ind), if cooperated and partner cooperated, defect from then on *, < 0, ≥ 0, ≤ 0, ≤ 0, 0, *, 1, with (|cc| > cd), *, < 0, ≥ 0, ≤ 0, ≤ 0, 1, 1, * *, < 0, ≥ 0, ≤ 0, ≤ 0, [1, 2, 0] / [1, > 1, 1], with (|cc| > cd)
Grim (Grim Trigger)	cooperate on the first move, if cooperated and partner cooperated, cooperate, if cooperated and partner defected, defect from then on

	100, ≥ 0, < 0, ≤ 0, ≤ 0, 0, *, 1, with (|cd| > cc), 100, ≥ 0, < 0, < 0, < 0, 1, 1, * 100, ≥ 0, < 0, < 0, < 0, [1, 2, 0] / [1,> 1, 1],with (|cd| > cc)
probGrim (probabilistic Grim)	cooperate on the first move with probability ind, if defected, defect (at dc/dd = 0 with probability (100-ind)), if cooperated and partner cooperated, cooperate (at cc = 0 with probability ind), if cooperated and partner defected, defect from then on *, ≥ 0, < 0, ≤ 0, ≤ 0, 0, *, 1, with (|cd| > cc), *, ≥ 0, < 0, ≤ 0, ≤ 0, 1, 1, * *, ≥ 0, < 0, ≤ 0, ≤ 0, [1, 2, 0] / [1, > 1, 1], with (|cd| > cc)
Impression-based strategies	
iApologizer (Adding, ConAdding, Averaging, ConAveraging)	defect on the first move with probability (100-ind), if defected, cooperate, if cooperated and partner cooperated, defect, if cooperated and partner defected, cooperate (at cd = 0 with probability ind) *, < 0, ≥ 0, > 0, > 0, [0, *, *] / [1, > 1, *]
iCooperator (Adding, ConAdding, Averaging, ConAveraging)	defect on the first move with probability (100-ind), if defected, cooperate, if cooperated and partner cooperated, cooperate (at cc = 0 with probability ind), if cooperated and partner defected, defect *, ≥ 0, < 0, > 0, > 0, [0, *, *] / [1, > 1, *]
iTemptation (Adding, ConAdding, Averaging, ConAveraging)	defect on the first move with probability (100-ind), if defected and partner cooperated, defect (at dc = 0 with probability (100-ind)), if defected and partner defected, cooperate, if cooperated, cooperate (at cc/cd = 0 with probability ind) *, ≥ 0, ≥ 0, ≤ 0, > 0, [0, *, *] / [1, > 1, *]
iShy (Adding, ConAdding, Averaging, ConAveraging)	defect on the first move with probability (100-ind), if defected and partner cooperated, cooperate, if defected and partner defected, defect (at dd = 0 with probability (100-ind)), if cooperated, cooperate (at cc/cd = 0 with probability ind) *, ≥ 0, ≥ 0, > 0, ≤ 0, [0, *, *] / [1, > 1, *]
iAntiTFT (Adding, ConAdding, Averaging, ConAveraging)	defect on the first move with probability (100-ind), if partner cooperated, defect (at dc = 0 with probability (100-ind)), if partner defected, cooperate (at cd = 0 with probability ind) *, < 0, ≥ 0, ≤ 0, > 0, [0, *, *] / [1, > 1, *]
iUnilateral (Adding, ConAdding, Averaging, ConAveraging)	defect on the first move with probability (100-ind), if defected and partner cooperated, cooperate, if defected and partner defected, defect (at dd = 0 with probability (100-ind)), if cooperated and partner cooperated, defect, if cooperated and partner defected, cooperate (at cd = 0 with

	probability ind) *, < 0, ≥ 0, > 0, ≤ 0, [0, *, *] / [1, > 1, *]
iMutual (Adding, ConAdding, Averaging, ConAveraging)	cooperate on the first move with probability ind, if cooperated and partner cooperated, cooperate (at cc = 0 with probability ind), if cooperated and partner defected, defect if defected and partner cooperated, defect (at dc = 0 with probability (100-ind)), if defected and partner defected, cooperate *, ≥ 0, < 0, ≤ 0, > 0, [0, *, *] / [1, > 1, *]
iTFT (Adding, ConAdding, Averaging, ConAveraging)	cooperate on the first move with probability ind, if partner cooperated, cooperate (at cc = 0 with probability ind), if partner defected, defect (at dd = 0 with probability (100-ind)) *, ≥ 0, < 0, > 0, ≤ 0, [0, *, *] / [1, > 1, *]
iLurer (Adding, ConAdding, Averaging, ConAveraging)	cooperate on the first move with probability ind, if cooperated, defect, if defected and partner cooperated, defect (at dc = 0 with probability (100-ind)), if defected and partner defected, cooperate *, < 0, < 0, ≤ 0, > 0, [0, *, *] / [1, > 1, *]
iGreedyTFT (Adding, ConAdding, Averaging, ConAveraging)	cooperate on the first move with probability ind, if cooperated, defect, if defected and partner cooperated, cooperate, if defected and partner defected, defect (at dd = 0 with probability (100-ind)) *, < 0, < 0, > 0, ≤ 0, [0, *, *] / [1, > 1, *]
iSucker (Adding, ConAdding, Averaging, ConAveraging)	cooperate on the first move with probability ind, if cooperated and partner cooperated, defect, if cooperated and partner defected, cooperate (at cd = 0 with probability ind), if defected, defect (at dc/dd = 0 with probability (100-ind)) *, < 0, ≥ 0, ≤ 0, ≤ 0, [0, *, *] / [1, > 1, *]
iGrim (Adding, ConAdding, Averaging, ConAveraging)	cooperate on the first move with probability ind, if cooperated and partner cooperated, cooperate (at cc = 0 with probability ind), if cooperated and partner defected, defect, if defected, defect (at dc/dd = 0 with probability (100-ind)) *, ≥ 0, < 0, ≤ 0, ≤ 0, [0, *, *] / [1, > 1, *]
Non-contingent strategies	
AllD (all defect)	always defect [0, *, *, ≤ 0, ≤ 0, *, *, *][†], 0, *, *, *, *, 1, 0, *
C_1–25	cooperate on every move with 1–25% [0 < ind ≤ 25, 0, 0, 0, 0, *, *, *][†], 0 < ind ≤ 25, *, *, *, *, 1, 0, *
C_26–49	cooperate on every move with 26–49%

	$[25 < \text{ind} \le 49, 0, 0, 0, 0, *, *, *]^{\dagger}$, $25 < \text{ind} \le 49, *, *, *, *, 1, 0, *$
Random	determine randomly whether to cooperate or defect $[50, 0, 0, 0, 0, *, *, *]^{\dagger}$, $50, *, *, *, *, 1, 0, *$
C_51–74	cooperate on every move with 51–74% $[50 < \text{ind} \le 74, 0, 0, 0, 0, *, *, *]^{\dagger}$, $50 < \text{ind} \le 74, *, *, *, *, 1, 0, *$
C_75–99	cooperate on every move with 75–99% $[74 < \text{ind} \le 99, 0, 0, 0, 0, *, *, *]^{\dagger}$, $74 < \text{ind} \le 99, *, *, *, *, 1, 0, *$
AllC (all cooperate)	always cooperate $[100, \ge 0, \ge 0, *, *, *, *, *]^{\dagger}$, $100, *, *, *, *, 1, 0, *$
D_1st_1–25C	cooperate on the first move with 1–25%, defect from then on $[0 < \text{ind} \le 25, < 0, < 0, < 0, < 0, *, *, *]^{\dagger}$
D_1st_26–50C	cooperate on the first move with 26–50%, defect from then on $[25 < \text{ind} \le 50, < 0, < 0, < 0, < 0, *, *, *]^{\dagger}$
D_1st_51–75C	cooperate on the first move with 51–75%, defect from then on $[50 < \text{ind} \le 75, < 0, < 0, < 0, < 0, *, *, *]^{\dagger}$
D_1st_76–100C	cooperate on the first move with 76–100%, defect from then on $[75 < \text{ind} \le 100, < 0, < 0, < 0, < 0, *, *, *]^{\dagger}$
C_1st_0–24C	cooperate on the first move with 0–24%, cooperate from then on $[0 \le \text{ind} < 25, > 0, > 0, > 0, > 0, *, *, *]^{\dagger}$
C_1st_25–49C	cooperate on the first move with 25–49%, cooperate from then on $[25 \le \text{ind} < 50, > 0, > 0, > 0, > 0, *, *, *]^{\dagger}$
C_1st_50–74C	cooperate on the first move with 50–74%, cooperate from then on $[50 \le \text{ind} < 75, > 0, > 0, > 0, > 0, *, *, *]^{\dagger}$
C_1st_75–99C	cooperate on the first move with 75–99%, cooperate from then on $[75 \le \text{ind} < 100, > 0, > 0, > 0, > 0, *, *, *]^{\dagger}$
probCoop (probabilistically cooperating strategy)	cooperate on every move (at cc/cd = 0 with probability ind) $*, \ge 0, \ge 0, > 0, > 0, *, *, *$
ExtremeAlternate	defect or cooperate on the first move, if defected, cooperate, if cooperated, defect $[0\|100, < 0, < 0, > 0, > 0, 1, 1, *]^{\dagger}$,

	[0|100, < 0, < 0, > 0, > 0, [0, *, *] / [1, 2, *], with (cc = cd), (dc = |cc|), (dc = dd)]†
probExtremeAlternate (probabilistic ExtremeAlternate)	cooperate on the first move with probability ind, if cooperated, defect, if defected, cooperate *, < 0, < 0, > 0, > 0, *, *, *
probMoody (probabilistic Moody)	cooperate on the first move with probability ind, if cooperated, cooperate (at cc/cd with probability ind), if defected, defect (at dc/dd = 0 with probability (100-ind)) *, ≥ 0, ≥ 0, ≤ 0, ≤ 0, *, *, *
probDef (probabilistically defecting strategy)	defect on every move (at dc/dd = 0 with probability (100-ind)) *, < 0, < 0, ≤ 0, ≤ 0, *, *, *

Note. The chromosome implementation gives the parameters for the genes ind (probability to cooperate on the first move and when indifferent, i.e., impression index = 0), cc (weight for CC on the previous move), cd (weight for CD), dc (weight for DC), dd (weight for DD), ada (0 = adding, 1 = averaging), ws (window size), and rec (0 = regular form, 1 = contradictory form). For impression-based and probabilistic non-contingent strategies, the description is only approximate, as the way the gene parameters are implemented into an impression index (Adding, ConAdding, Averaging, ConAveraging) determines the actual behaviour. With 1-step memory strategies, chromosome categorizations of strict and probabilistic form often overlap (e.g., Apologizer corresponds to one of the probApologizer categorizations with ind = 0), so we implemented the strategies in the given order (from specific to general) to make sure all strategies are categorized properly. * The parameters for the respective genes are irrelevant. † The chromosome implementation is omitted with the onset of noise.

Figure C1

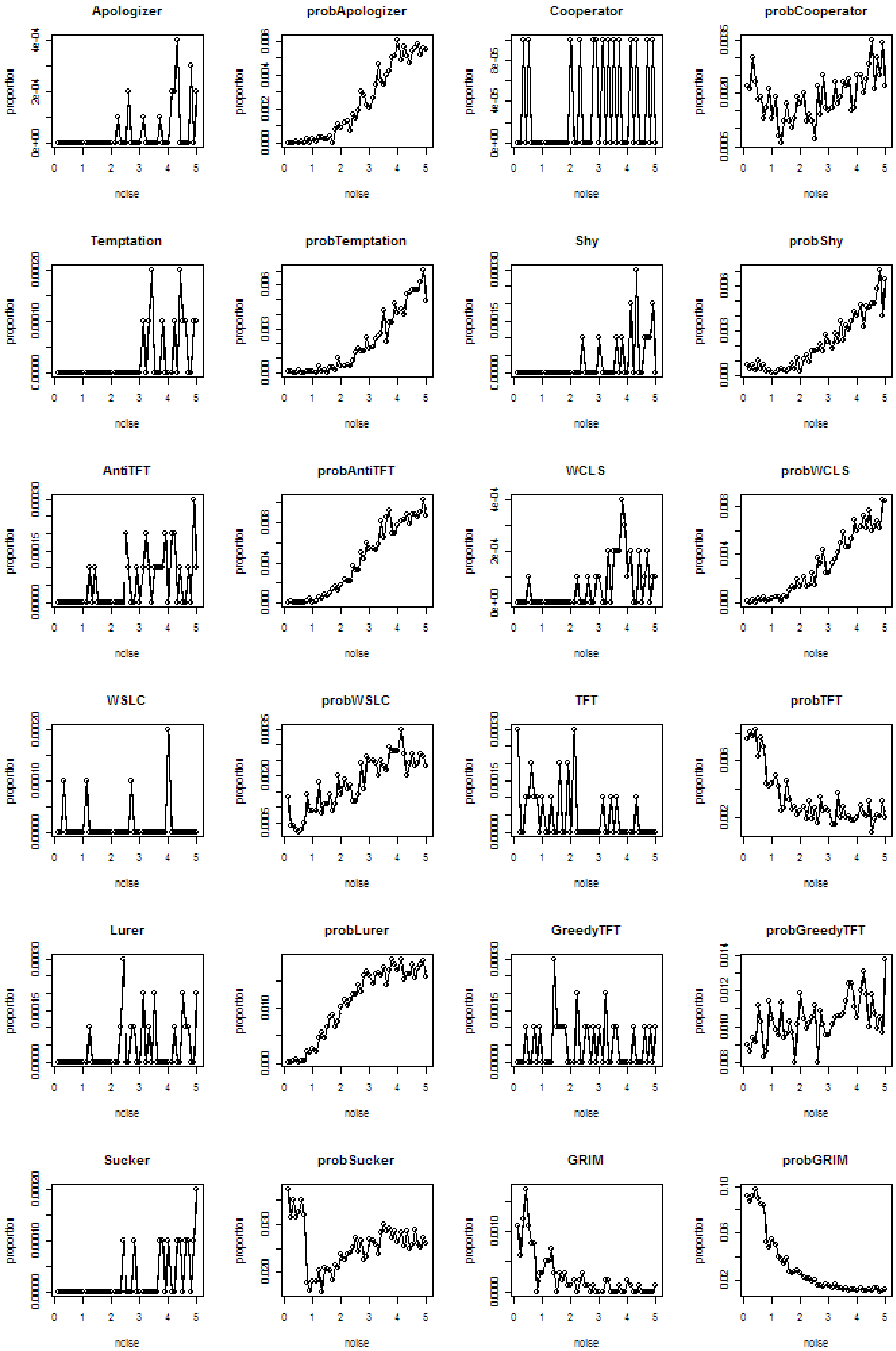


Figure C1. Proportion of 1-step memory strategies winning among 10,000 runs over an increasing level of noise (represented by the standard deviation of a normal distribution around the correct impression index/weight).

Figure C2

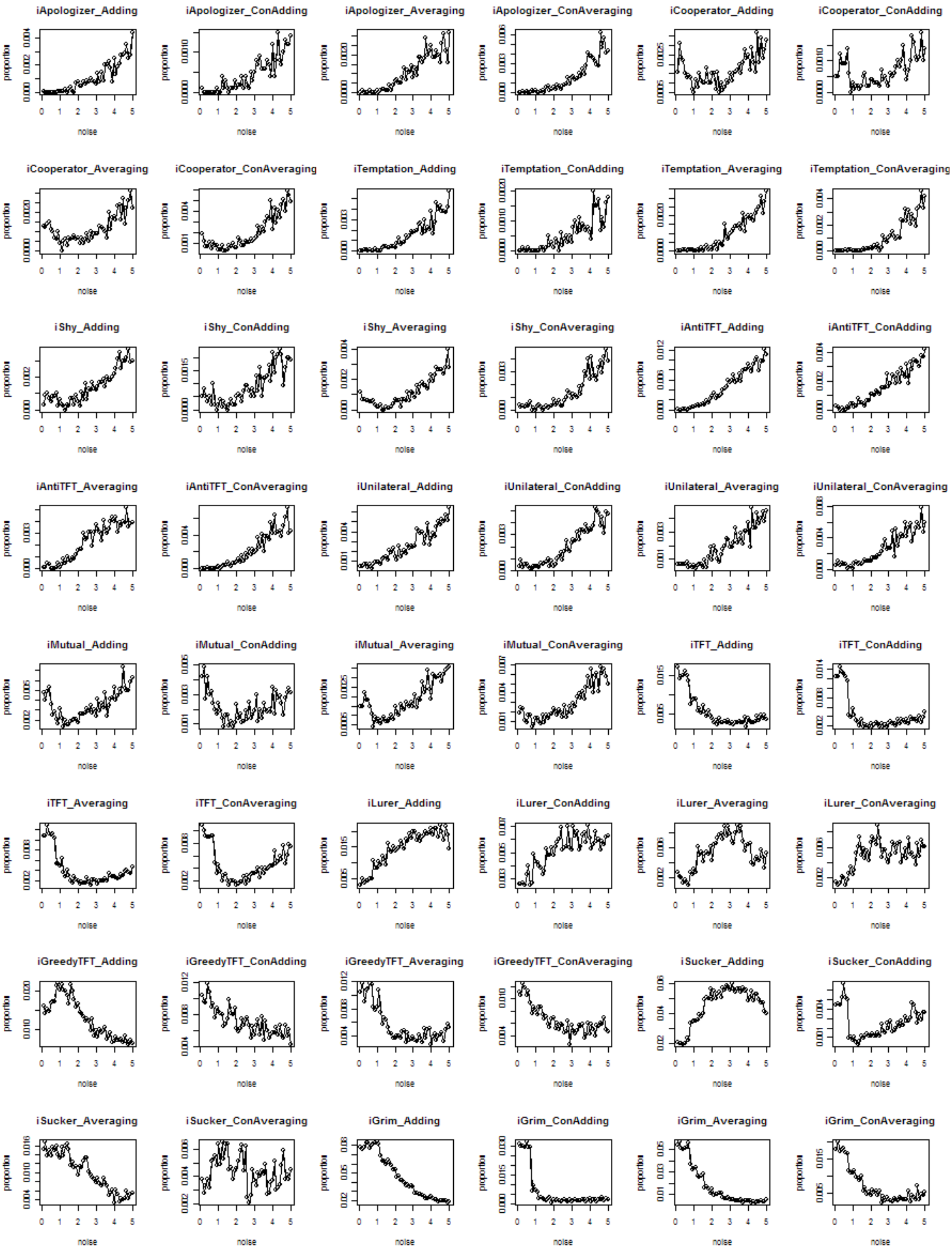


Figure C2. Proportion of impression-based strategies winning among 10,000 runs over an increasing level of noise (represented by the standard deviation of a normal distribution around the correct impression index/weight).

Figure C3

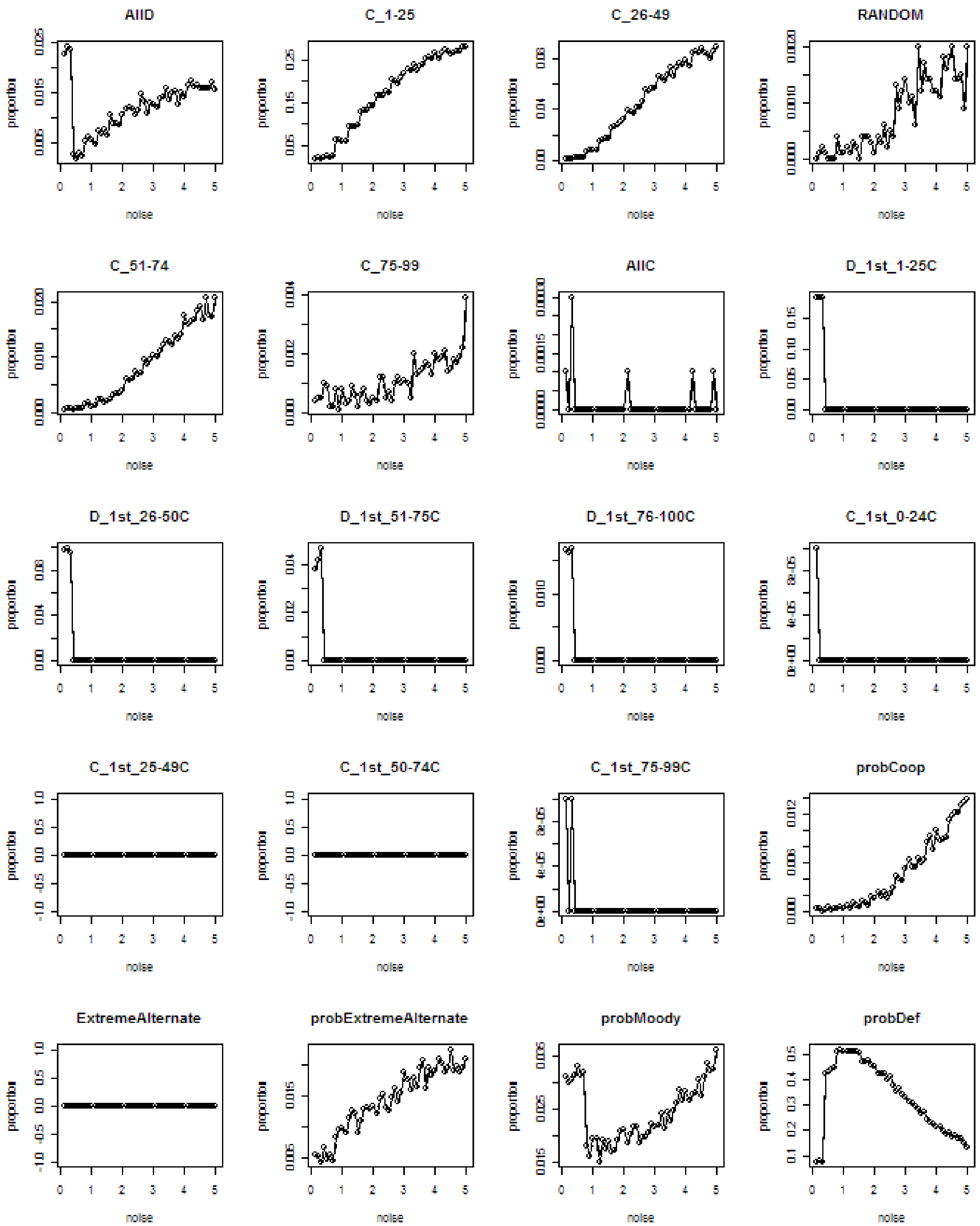


Figure C3. Proportion of non-contingent strategies winning among 10,000 runs over an increasing level of noise (represented by the standard deviation of a normal distribution around the correct impression index/weight).

Selbständigkeitserklärung

Ich versichere, daß ich die Dissertation selbständig angefertigt habe. Sie ist auf der Grundlage der angegebenen Literatur und der weiteren Quellen gewachsen, mit Betreuung durch Prof. Jörg Rieskamp (Universität Basel) und Assistant Professor Dr. Jeffrey R. Stevens (University Nebraska-Lincoln), in Zusammenarbeit mit meinen geschätzten Mitautoren und Gesprächen mit meinen Kollegen der Adaptive Behavior and Cognition-Forschungsgruppe (ABC) unter seinem Direktor Prof. Gerd Gigerenzer (Max-Planck-Institut für Bildungsforschung, Berlin) sowie den Kollegen der International Max Planck Research School "Adapting Behavior in a Fundamentally Uncertain World". Die Rekrutierung von Versuchspersonen und Durchführung der Experimente übernahmen Gregor Caregnato und seine Kollegen vom ABC-Labor. Die bei der Umsetzung benutzten Programmiersprachen habe ich angegeben und darüberhinaus keine unerlaubten Hilfsmittel verwendet.